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ART. I.—*A Notice of Prof. Augustine Pyrame De Candolle;* by GEORGE B. EMERSON, Pres. of the Boston Soc. of Nat. Hist.

AT a meeting of the Boston Society of Natural History, held in their hall, Nov. 17th, 1841, the President read portions of a letter from his friend, Edward Tuckerman, Jr., containing, together with many interesting facts relating to the botanists of England and France, the melancholy intelligence of the death of DE CANDOLLE. After reading this letter, the President went on to say:

Thus has set one of the great lights of botany—a man, who, for the vastness of his works and the comprehensive idea he had formed of the extent and ends of the science, has not left his superior, hardly his peer. May I be allowed to take this occasion to say a few words upon the works and character, as a botanist, of the man whose loss we are thus called to deplore.

AUGUSTINE PYRAME DE CANDOLLE was born in Geneva in 1778, of an ancient family, which, as long ago as the sixteenth century, was distinguished in the republic of letters. From his earliest years he seems to have devoted himself to botany; for already in his twenty first year, in 1798, he published his History of Succulent Plants.* In the preface to this work he asks, in the sim-

* Plantarum Historia Succulentarum, published in Paris by Dugour and Durand, in two folio volumes with colored plates. This contains descriptions of succulent plants, not of any one natural order, but of those having sufficient resemblance to be associated. They are mostly of the orders Crassulaceæ, Ficoideæ, and Cactaceæ, with some species of aloes, and a few others. They are precisely those plants of which it is important to have good figures, as of most of them it is nearly

revisal, in order to render it a correct representation of the actual state of human knowledge, with such additions
s the progress of discovery during its publication have rendered necessary; additional references will also be

plest manner, indulgence for the youth of its author, at the same time admitting that every book must plead its own cause, and promising to endeavor to make up in zeal for the deficiencies of his experience and knowledge. There certainly could have been no affectation in this modesty, for it shows itself just as clearly in every future work; and that zeal must have been no less real which held out to the last years of his laborious life.

Two years after, in April, 1800, he laid before the National Institute, in manuscript, his Astragologia.* This was committed to Lamarck and Desfontaines, at that time two of the most distinguished botanists of France, and who ever after seem to have been his firm and honored friends. They reported very favorably upon the work, observing particularly upon the extent of his researches, and the exactness and precision of his descriptions. From this time he began to be well known, and from this, too, probably, dates his connection with Lamarck, with whom he was afterwards associated in editing the Flore Française. His connection with Lamarck and with the Flore Française, was of momentous consequences to him. It was the first edition of this work, as he himself declares,† which, by initiating him in his youth into the elements of botany, decided his taste and his fate for life.

It must have been about this period that he spent six years in traversing all the provinces of France.‡ In every one he herborized; every where he studied the vegetation, and every where made, or obtained from public or private collections, specimens and documents.

In 1804, he published in quarto his Essay upon the Medical Properties of Plants. It was his inaugural thesis on taking the degree of doctor in medicine in the Faculty of Paris.

In the same year he delivered his first course of lectures on physiology, the substance of which he introduced in the "Princi-

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impossible to make dried specimens. Each is accompanied by a beautiful colored figure by Redouté. If it were intended, as it seems to be, to give a pretty full history of plants of this character, a comparison of it with the account of the same genera in the Prodromus, will show how immensely the species increased in number in the interval which elapsed until the publication of the latter work.

* Astragologia was published in Paris in 1802, in one volume folio, by Garnery, from the press of Didot. It contains descriptions of a great number of species, many of them new, of Astragalus and the allied, often gum-bearing Leguminosæ. It is illustrated with fifty plates by Redouté.

† Preface to the sixth volume of Flore Française, p. 10.

‡ Flore Française, vi, preface, p. 7.

ples of Botany" prefixed to his edition of the Flore Française, which was published immediately after. In this edition he added the surprising number of two thousand to the species of plants (twenty seven hundred) previously described in the Flora,* and in the sixth volume of the edition of 1815, he makes an addition of thirteen hundred more, thus raising the number of species belonging to France, under the empire, when it comprehended the Italian and German provinces, to six thousand, about a fifth part of the total number of plants then known on the globe.

In preparing this last edition, he had called to his aid all the best botanists of France, increased to a large number by the publication of the former editions of this very work; and he makes express acknowledgment by name, to forty three for France proper, ten for the Italian provinces, and five for the German. These great additions were made with scrupulous care, for he introduces the description of no plant of which he had not an authentic specimen.

It is not easy to say how it has happened that the Flore Française has not attained a celebrity, *out of France*, more nearly proportioned to its preëminent excellence. It is, without question, the most complete Flora‡ that has ever been made; the Preliminary Discourse and the General Principles giving every thing that is necessary to an understanding of the work, and the "Analytical Method" presenting the only tolerable substitute since the time of Linnæus, for the artificial system of that great man, in solving the first question that always presents itself in examining a plant—what is its name? The very fact that a single editor could have enlarged, by more than one half, the flora of his native land, and that too the native land and the seat of the labors of Tournefort, the Jussieus and Lamarck, should have turned all eyes towards it. This side the ocean and the channel, indeed, as beyond, all eyes have been occupied with the treasures that have been flowing in from both Americas, New Holland, the Pacific islands, Farther India, and the coast of Africa; and the Browns, and Hookers, and Grevilles, and Lindleys of England, and the Nuttalls, Elliotts, Bigelows, Torreys and Grays of America,

* Flore Française, p. 1. † Ibid. p. 9.

‡ The English Flora of Sir J. E. Smith, is a mere flora, and takes it for granted that the reader has learnt the principles of the science from some other source. Besides which, it gives nothing of the natural orders.

have had enough to do to examine and describe the plants they have been gathering, or that have been sent home to them.

De Candolle did not wait to ask how his labors were received abroad. In 1808 he became an inhabitant of Montpelier,* and took charge of the botanic garden there, which he raised to the highest perfection. For ten years from this time, he must have been beyond measure diligent. He thoroughly explored the south of France, gave courses of lectures at the Faculty of Medecine in Montpelier, published, in conjunction with Lamarck, a synopsis of the plants of the French flora, gave a catalogue of the plants in the botanic garden of Montpelier, published figures and descriptions of the rarer plants of France, several articles on geographical and agricultural botany, in 1813 his Elementary Theory of Botany,† and, in 1816, a second edition in octavo, of his "Essay on the Medical Properties of Plants."

The object of this work,‡ is to ascertain the relations which subsist between the medical properties of plants, their external forms, and their natural classification. The dedication is curious:

"To the botanists who laid the foundation of the Theory of Natural Relations,—J. and G. Bauhin, Tournefort, Magnol, Ray, Morison, who had an anticipation of it; Bernard de Jussieu, who proved it; Adanson, who developed it; Antoine-Laurent de Jussieu, who subjected it to fixed laws; Desfontaines, who connected it with vegetable anatomy; Richard, who threw light upon it by the analysis of fruits; Robert Brown, who extended it by the examination of the plants of New Holland."

Considering the uniform justice and generosity of De Candolle, this dedication is remarkable for its injustice in omitting the name of Linnæus, to whom, as he confesses in this very work,§ is due the first perfectly distinct enunciation of the principle which it is the object of the "Essai" to prove—that plants of the same genus have the same properties; those of the same natural order, similar properties; and those of the same natural class, some analogy in their properties; and he admits that Jussieu *adopts*

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* Flore Française, vi, 7.

† Théorie Elémentaire de la Botanique. 1 vol. 8vo. A second edition was published in 1819.

‡ Essai sur les Propriétés Medicales des Plantes, comparées avec leurs formes exterieures et leur classification naturelle. Paris. Crochard, 1816.

§ Essai, Introduction, p. 4.

this opinion.* The omission is, moreover, inconsistent with the profound respect with which he always refers to the great systematist.

In the midst of and by means of this multiplicity of labor, De Candolle was commencing what he meant to be the great work of his life, and what, if he had lived to complete it as it was begun, would have been far the most remarkable work on botany ever executed—I mean his Natural System of the Vegetable Kingdom.† He seems to have entered upon it with a full sense of the greatness of the undertaking before him, and with a lofty and resolute, but ever modest purpose‡ of devoting his whole strength and time, with all his immense resources and all the accumulated ability of a life devoted to the preparation, to its accomplishment. One is reminded of the heroic perseverance with which Saussure, from the banks of that same beautiful lake of Geneva, had for so many years cherished the firm resolve to scale Mont Blanc, and make known the new and strange phenomena which its naked top should present; or of that more than heroic purpose, cherished and kept still at heart through a whole life of preparation, of the far greater Milton, to achieve something worthy to be remembered in future years, and to place his name with those of

> "Blind Thamyris and blind Mæonides,
> Tiresias and Phineas, prophets old."

De Candolle's plan was no less than to arrange and describe, in their natural orders, all known plants, giving to each order the fulness and completeness of a monograph. We who now look back upon his work, can see how far he soared above those whom he hoped to equal, Lamarck, Willdenow, Vahl, Persoon. Few works give us better executed examples of the Baconian method, of forming general conclusions from the careful observation of particulars, and thence going back and reëxamining the particulars. He first minutely examined all the species of a genus, and thence

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* After quoting Amœn. Acad. v, p. 148, for this opinion of Linnæus, he adds, "M. de Jussieu adopte la même opinion." Introduction, p. 5.

† Regni Vegetabilis Systema Naturale, sive ordines, genera et species plantarum secundum methodi naturalis normas digestarum et descriptarum. Volumen Primum, Parisiis, sumptibus sociorum Treuttel et Wurtz, 1818.

‡ Hanc ipsam curam, hoc opus quod nemo apud nos, nemo apud veteres tentavit, timidus hodie aggredior. Prolegomena, p. 3.

drew his generic characters. From a similar full examination of all the genera of an order, he drew the ordinal characters. This done, he returned to the genera and species, and rejected from the generic what had been sufficiently expressed in the ordinal, and from the specific, what had been distinctly stated in the generic characters. Thus, applying the highest logic to his work, he gave a model both of analytical and synthetical investigation, which has done much to raise botany to the rank which it holds among the sciences, and set an example by which every succeeding botanist has profited.

Besides this general preparation, which involved many years of diligent and methodical labor in his study, he made special preparation which few, up to that time, had attempted. He visited and carefully examined all the herbaria, even at that time immense, of France and England. He noted their contents; and obtained the coöperation of nearly all the distinguished botanists then living. Nor was it with living botanists and with herbaria alone that he had to do. He studied with minute and patient care whatever had been previously written of plants. For evidence of the extent of his investigations, we have only to refer to his history of almost any plant he has described. For *Hepatica triloba*,* for example, he refers to volume and page, and accompanying figure, if any, of more than thirty descriptions before the Species Plantarum of Linnæus; to fifteen more under the name given in that work; to six additional ones, under other names; to five more for the American variety; and he had besides examined six authentic specimens! Thus we have reference to full sixty descriptions and six herbaria for a single plant.

The first volume of the Systema was finished in Geneva, to which place he had in this busy interval removed, interrupted, it is said, in the midst of his peaceful labors, by the demon of party spirit. It bears the date of October, 1816. It contains only five orders.† A second volume, containing six orders,‡ and finished in the same elaborate manner, appeared in 1821.§

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* Vide Reg. Veg. Vol. I, p. 216.

† Ranunculaceæ, Dilleniaceæ, Magnoliaceæ, Anonaceæ, and Menispermeæ.

‡ Berberideæ, Podophylleæ, Nymphæaceæ, Papaveraceæ, Fumariaceæ, and Cruciferæ.

§ A beautiful quarto volume of illustrations of the rarer and more curious plants described in the first volume of the Systema, especially of those in the herbarium

But De Candolle had undertaken a work perhaps beyond the strength of any man, whatever might be his capacity; and I believe no other volume has appeared.

In November, 1823, he finished, at Geneva, the first volume of a work of infinitely less pretensions, undertaken at the urgent request of his friends, which he offered and intended only as a rapid survey of plants, to precede his great work, the completion of which he still kept in view. He gave it the modest title of Prodromus.* The first volume, comprehending fifty four orders, was published in 1824.†

In the next year came the second volume, containing only ten orders, showing that his materials were rapidly accumulating under his hands, and that he had imperceptibly enlarged his plan. This was accompanied by a volume of Memoirs on the Leguminosæ,‡ with numerous plates. In 1828, '30, '36, '37 and '38, appeared successive volumes of the Prodromus, and within this period, ten memoirs on various subjects, with plates, now collected in one volume.§

During all this time, his lectures were going on at the Museum in Geneva; among others, a course on agricultural botany, of the substance of which we have some portion in the graceful dress in which it is presented by Mrs. Marcet in that admirable volume which she called "Conversations on Vegetable Physiology."‖

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of De Lessert, was edited by B. De Lessert, with one hundred plates by Turpin, and published in Paris in 1820. A similar volume, still better executed, also under the direction of Turpin, and with one hundred of his engravings, and many beautiful dissections, was published by De Lessert in 1823, in illustration of the second volume of the Systema.

* Prodromus Systematis Naturalis Regni Vegetabilis. Parisiis: Treuttel et Wurtz. Pars Prima, 1824.

† In 1837, De Lessert published a third volume of Illustrations in the same style and of the same size of those just noticed, containing figures of some of the rarer plants in the first four volumes of the Prodromus, together with those of others not described in that work.

‡ Mémoires sur la Famille des Légumineuses. Paris: 70 planches, 4to. pp. 525.

§ Collection de dix Mémoires pour servir à l'Histoire du Regne Végétal. Paris: Treuttel et Wurtz, 1828—1838.

‖ This has been published in this country under the title of "Blake's Botany" by some person who has thus claimed a property of authorship in the book, on no better ground than his having interspersed questions which give not the slightest intimation of an acquaintance with the subject, and leave one doubtful whether he knew enough of it to distinguish a sedge from a bulrush, or a moss from a lichen. Would that this shameless kind of piracy were confined to a solitary case.

In 1826 he had prepared for publication his work on the organs of plants,* another portion of his course of botanical lectures. Rich as they had become from his extensive reading, from observation, and from the constant suggestions of his vigorous and original mind, it was no longer just to withhold them from the public. In this work on Organography, following out the hint given by the German poet Goëthe,† taking advantage of the light afforded by those who were successfully engaged in exploring the animal kingdom, and gathering conclusions from the immense number of new facts presented by other botanists, and by himself in preparing the Prodromus, he exhibits a fuller and more philosophical view of vegetable structure, than had previously been given, in the language.

Five years after, in 1831, he completed his great work on Vegetable Physiology,‡ also taken from his lectures, a rich storehouse of facts, upon the properties and functions of the organs of plants and the forces external and internal which act upon them. The title of Physiology he admits to be not the most proper which could be devised; and he would have preferred that of Organodynamy—the forces of the organs, as more descriptive of the subject of his treatise.§

One of the most interesting portions of this work, at least for the young philosophical student of botany, is the Appendix; marking out, as it does, the limits of the science in its most important particulars, and indicating to the physiologist, the traveller, the cultivator, the chemist, and the natural philosopher, as well as to the botanist, to what points it is important that attention should be directed, to advance still farther the boundaries of our knowledge. How many inquirers have these questions already stimulated to action! Many of these questions he would

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* Organographie Végétale ou Description Raisonnée des Organes des Plantes. Deterville, Paris, 1827. 2 vols. 8vo.

† Org. Veg. p. 8.

‡ Physiologie Végétale, ou Exposition des Forces et des Fonctions vitales des Végétaux. Paris, 1832. 3 vols. 8vo.

§ According to the idea of De Candolle, vegetable physiology is but one department of general physiology. It cannot be fully comprehended by one who is ignorant of its principles in their other forms. Adequately to understand the nature of plants, he must study atmospherical influences, the action of light, electricity and heat, the laws of chemistry, the nature of soils; and whatever can be known of the laws of life.

himself have solved, but feeling that he had laid out more work than the longest life could suffice to execute, and anxious to complete the Prodromus, he frankly imparts his plans, his doubts, and his suspicions, hoping that younger and less busy laborers would clear up the one and complete the other.

He more than once intimates his intention of following the publication of the Physiology with other works which should cover the whole ground he had gone over in his lectures. In the preface to the Physiology, he gives the titles of those which he intended, if time and health and will should serve, to send forth, to fill up the great plan he had laid down. To complete the *fundamental* portions, in addition to the two works he had already published, he proposed to himself *methodology*, which should deduce from the study of the organs, the principles and methods of classification. For the principles on which this was to be executed he refers us to the "Elementary Theory;" for the conclusions, to the Prodromus.

Among the accessory branches of the science, he includes,

1. *Botanical Geography*, which, from the two preceding, infers the laws or general facts relative to the distribution of plants on the surface of the earth;*

2. *Oryctological Botany*, which would comprehend the history of fossil vegetables, considered in their relations both to the strata of the earth and to the forms of recent plants;

3. *Historical Botany*, exhibiting the steps by which botany has arrived at its present state.†

Among the *practical parts*, he reckons,

1. *Agricultural Botany*, the application of the principles of the preceding to the culture of vegetables, on which he had twice given a course of lectures;

2. *Medical Botany*, their application to medicine; on which subject he apparently meant to enlarge the work already mentioned, upon the medical properties of plants;

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* On this subject he had published, in 1809, the article "Botanical and Agricultural Botany," in the Dictionnaire d'Agriculture, and in 1820, the article "Botanical Geography," in the Dictionnaire des Sciences Naturelles, besides the introduction to the second volume of Floré Française, explaining the botanical map of France.

† The article *Phytography*, in the Dictionnaire Classique d'Histoire Naturelle, contains an outline of this portion, extracted from his lectures upon the subject.

3. *Economical Botany,* which was to comprehend the study of all the other modes of making plants subservient to the wants of man; a part of the subject which he looked upon as still to be written.*

Such are some of the works he contemplated, which he had long familiarly considered, and for which his whole course of study had been a preparation. In what words can we sufficiently lament the loss to science and to humanity of a man who had laid out so broad a plan of useful labor, and had given such examples of the manner in which he was hastening to complete it! It is to be hoped that the manuscript notes of the extensive courses of lectures to which he refers, may supply his son, or some other equally competent person, with the means of filling up the outline he has sketched.

Eight years of unremitted and obstinate labor had been consecrated to the study of the immense family of the Compositæ.† This intense devotion, the natural effect of his native and characteristic ardor, proved too much for his health, already affected by severe study, and undermined, it is said, by a constitutional malady. What constitution could have held up under labors so immense? We know not the particulars of his end; the burden was too great, and the body of the wise and strong man failed.

It is impossible to estimate the extent and magnitude of the influence of the writings and character of such a man as De Candolle. The science to which he devoted his life must always feel it; it cannot go back. His vast plan once laid down, noble spirits every where and in all future time will strive to realize it. His great idea of the science once spread before the world, no one can hereafter aspire to the worthy name of botanist, who is a mere collector and labeller of specimens, or a mere dissector of plants. He must aim to fill his mind with the extensive and various and exact knowledge of other sciences, and of all parts of his own, which this great man has shown to be essential to an accomplished botanist.

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* From an article on the geographical distribution of the plants used as food, in the "Bibliothèque Universelle de Genève," for April and May, 1836, we conceive hopes that M. Alphonse De Candolle is engaged in a work of this kind.

† See Mémoire Neuvième, in the volume of Mémoires. If the Compositæ had been given with the same fulness and minuteness of description and reference which characterize the Systema, they would have occupied twelve or fifteen volumes.

Gathering our materials only from his works, there is very much which we should delight to know, in regard to De Candolle, of which we are now almost entirely ignorant. We would gladly learn what was the discipline and what were the studies of his early years; whence he gained his beautiful and simple Latinity, whence the clearness and elegance of his style, and his severe and exact logic; by what wise arrangement of his hours he accomplished so much, and made such attainments in the knowledge, not only of living plants, but of herbaria, and books, and various sciences. We know him as a philosopher and a botanist, and we understand and feel the power of his mind and the force of his genius. We would gladly see him in the higher relations of friend, and father, and citizen—we would know him as a man. We hope that we shall not long remain without a life of him by some one capable of understanding his works and appreciating his character.

ART. II.—*Geological Reports of the State of New York for* 1840, *communicated by the Governor to the Assembly, Feb.* 17, 1841. [Assembly, No. 150.]*

BY these reports of the geological corps, the survey of the state is nearly completed, and after a partial continuance of the work for this season, the final report is to be made. The desired work on the geology of this great state may be anticipated in the beginning of the next year. It is generally understood that the final report will be made by the corps in the manner already done. For the mineralogy, zoology, botany, and palæontology of the state, this is the proper course. It is to be hoped, however, that the geology will not be divided into four great districts, but that the maps, and sections, and descriptions, will present before the public one general view. This is the more important, as the four districts assigned to four principal geologists have no natural geological lines of separation, but are connected in the most intimate manner. The importance of a chief geologist, or of the most complete harmony of views, becomes obvious, extending, as do several of the strata or groups, through more than

* Communicated for this Journal by the author.

one of the districts, the *designations* of the rocks, their *position, connection,* and *organic remains,* demand the language of one mind. Hoping for a harmonious and splendid work, let us take a brief review of the progress of the survey for the last year.

As the general groups had already been announced, and are considered correct, the additions have been made chiefly from the filling up of the less observed parts of the state.

1. Facts in respect to the *salines* at Onondaga lake. By boring at Syracuse to the depth of two hundred and sixty five feet, one hundred feet deeper than any previous boring, brine of much greater strength was obtained. Taking saturation at 100°, the old well yielded brine of the strength of 56°, on the scale; while the new well gives the strength of 78°, a difference of nearly forty per cent. The average number of bushels produced in the two preceding years, was 2,700,000; and this amount may now be greatly increased. The importance of the salines to the state and to the country, needs no remark, and the means of making them more valuable are ably considered by Dr. Beck, pp. 18–23, and by Prof. Vanuxem, pp. 141–5.

2. The amount of hydraulic limestone in the western part of the state, and of nearly the same excellent quality. It lies at the base of the terrace of limestone, which is so prominent in Erie County, and may be traced from Niagara River to Cayuga Lake. It is attended every where "by numerous and copious sulphur springs," or springs yielding sulphuretted hydrogen gas. From Black Rock eastward, it occurs in many places; abounds at Williamsville, where it is burned and ground for cement by thousands of barrels. It occurs at Rochester also. The difference in the quality of the hydraulic lime is attributed more to the *burning* than to the stone itself. Many of the strata appear to grow thinner in the western district, and some to disappear. See Report, pp. 150–8, Hall. This terrace bounds on the south all the beds of gypsum. p. 156.

3. Discovery of abundance of primary limestone on the north and the west shores of Lake Janet, is of great importance to that part of the state.—*Report,* p. 126, Emmons.

The "steel ore" of the Duane bed is wrought at once into cutting instruments of fine quality. Prof. Emmons does not state that the ore is *steel,* or that it is made into steel, but the fact that good edge tools are formed of it. p. 134, 135.

4. The Trenton limestone is an important rock for its marble, in Schenectady County, and particularly in Saratoga County. It is a fine grained and durable stone. On one of the strata of it at Glen's Falls on the Hudson, the upper surface contains marks "like those produced in soft mud by drops of rain."—*Report*, p. 97, Mather. *Ripple* marks are shown on "brown sandstone on the road from Cattskill to the Mountain House, one mile above the tollgate." p. 85. The *chloritic* and talcose rocks of the western part of the valley of the Hudson, are considered by Prof. Mather, as "*metamorphic* with intrusive rocks interstratified:" and he extends the metamorphic rocks into the western part of New England. pp. 93 and 96. In this opinion he is opposed by some of our distinguished geologists, and cannot be followed without far more and much stronger proof than has yet been offered. Indeed it seems obvious that the metamorphic theory is an easy method of breaking down the usual distinctions of the rocks. A fine section of the rocks "from the summit of the Cattskill Mountain, south of the Mountain House, to Cattskill Creek," is given on pages 78–81, *Mather*.

5. The report of the paleontologist contains very important matter, and some interesting corrections.

Mr. Conrad distinguishes our rocks of the Silurian system, "for the sake of convenience," into three subdivisions, the *Lower, Middle, and Upper, Silurian series;* it seems also to be quite a natural arrangement. The danger is that the classification has been made too early, and that adequate examination has not settled the entire distinctness of all the series. A great proportion of them however are satisfactorily ascertained. There is some doubt about the termination upwards of the *Lower* series. It is limited by the *Pentamerus oblongus limestone* in the report, and the lowest stratum in the *Middle* is made Rochester shale. If we understand it, this *Pentamerus limestone* at Rochester is towards the lower part of the shale, and at Lockport is found entirely above the Rochester shale, which is much thicker there than at Rochester; there would be no difficulty in the natural arrangement in making the Red or Medina Sandstone the upper stratum in the *Lower* Silurian. But the *Pentamerus limestone* must go with it, in whatever series it is placed.

On p. 31 is a tabular view of the whole series, with the corresponding rocks of Murchison, and the characteristic genera of

organic remains. The Rochester and gypseous shales are given as the equivalent of the *Wenlock Shale* of Murchison. The fossils placed against Nos. 7, 8 and 9, belong to the lower rocks, as Triarthrus and Isotelus to the Trenton limestones. Against No. 9 should stand the shell *Pentamerus oblongus*, or if a trilobite must be introduced, it can be neither of those mentioned, but *Asaphus longicaudatus*. Where is the statement that the remains against Nos. 5–9 have been found even as high as the Rochester shale?

The limestone of Tully, or Tully limestone, is considered as identified with the *Aymestry limestone* of Murchison by two species of shells, *Avicula reticulata* and *Atrypa didyma*. p. 31.

The fronds of Fucoides are often very prominent, but the structure like that of the rock in which they exist; of other plants, scarcely more than impressions remain. The petrifying material of the corals is commonly *siliceous* earth; but of the Crinoidea, is *limestone*, whether they lie in calcareous or siliceous rock. p. 40.

Several new genera and many new species are described, and many yet remain for the final report. pp. 48–57.

Several important corrections are announced.

The *Calymene Blumenbachii*, as the trilobite in the Trenton limestone was called, Mr. Conrad considers a new and undescribed species; it is named by him *Calymene senaria*, from its place in so old a rock. It no longer is an instance of an animal that "escaped into remote seas," and lived in a much later era than its period of destruction here. "*Calymene platys* of Green" is stated to be *C. Blumenbachii*, and to agree with it in *place* also; so that this fossil is found where it *ought to be*, and no longer throws uncertainty over the age of our rocks.

The evidence is now thought conclusive by Mr. Conrad *against the existence* of any fresh-water shells in the strata below the carboniferous, "in which Uniones occur in Pennsylvania." The supposed Unio in the sandstone at Medina turns out to be something else, the Planorbis "probably a Bellerophon," and another to belong to an allied genus. This correction is the more necessary as these shells "are associated with two marine genera, Lingula and Orthocera." Probably the existence of fresh-water fossils in that sandstone had been a matter of high surprise to most geologists; the evidence decides the case. p. 41.

In former reports, Mr. Conrad has spoken of the *Cambrian* system below the *Silurian*, as being developed along the eastern part of the state, but in this report he speaks of the Silurian system as "composed of the oldest fossiliferous rocks yet discovered in North America." p. 26. No reason is given for this change; whether Mr. C. now refers these rocks to the Silurian system itself, or considers them as *metamorphic* rocks of this formation. He states "the oldest fossiliferous rock hitherto known" in our country to be the "calciferous sandstone" of Eaton, which contains "two species of Lingula, a small *univalve*, and something resembling *fucoidal* remains." p. 28. Prof. Eaton speaks of organic remains in a rock below this, and others have judged the same. The final report will doubtless clear up this matter, which seems rather obscure. It does not follow because some remains have been found, that much more extensive and particular examination will not discover others in the same or associated rocks, till we pass beyond the region of petrifactions.

"The mixture of species" sometimes occurs "at the junction of two formations," (groups.) This fact shows the necessity of great care in the division into groups, and renders the "exact line of demarcation" between them somewhat uncertain. It is not necessarily opposed, however, to the notion that "sudden convulsions of the earth's surface" have caused the destruction of most of the existing forms of life, nor does certainly prove that the temperature of the ocean has suddenly or gradually changed, because it is easy to see that many individuals *might for a time withstand* sudden or gradual changes or convulsions, unless it can be shown, as it cannot, that the convulsions were so great that the then existing forms of life must be destroyed by them. The continuance of some species through several successive groups or formations, is a fact of similar consequence, and admits the same easy solution; it certainly is no obstacle to the stratigraphical arrangement, because such species have no *characteristic* value, they designate or *characterize* no particular group,—they belong to no particular rock, and need no minute observation to give them their due estimation. p. 26.

There is a correction also of a former statement opposed to the notion of the impression of birds' feet in sandstone. The terminations of the Fucoids "sometimes rudely resemble a human hand, whilst others are not unlike the foot-marks of birds." p. 33.

Though these are maintained to be *fucoidal* remains in our sandstone; it is conceded, that the "curious foot-marks," the *Ornithichnites* described by Prof. Hitchcock in the new red sandstone, "peculiarly characterize that system." p. 43. This harmony of opinion is an indication of the firm foundation of this part of the science.

The shells formerly referred to the genus *Terebratula*, are said to belong to other and extinct genera. The mistake has not been made by our own geologists alone, if mistake it is. The shell remains, and a name so distinguished as that of Sowerby, sustains it. True it may be, that what is now defined to be a *Terebratula*, may not exist in our rocks; but to mention no more, *Delthyris* (*Terebratula*, Sow.) *tripartita* seems to be a common and widely spread species. p. 35.

A conclusion drawn from the state of the organic remains, has great interest, viz. that the depositions took place "in the bed of an ocean, undisturbed by violent currents or greatly agitated waters." This is derived from the finely preserved parts of even fragile shells, only one stratum being known in the state as an exception, "where the shells are in a fragmentary condition." p. 26, 7. This is true also, as we know from a previous report, of only a small portion of the Pentamerus in the limestone at Rochester, the rock referred to. As in many others the "valves of bivalves are found apart," so are they in this; often changed so that the hinge is fitted to the mouth; often the valves lie crosswise; more often piled above each other and petrified in masses, the convex matched into the concave side of another valve; often entire shells with their valves unmoved; and, often the surface of the stratum and sometimes the interior, with fragments of valves connected in any way. The indication of a disturbing power is here considerable. Is not the evidence far greater in the case of the *encrinal* limestone of Lockport, and the thin stratum of it at Rochester, where the fragments of the stems of encrinites are promiscuously jumbled together, and the surface made up of portions of shells, porites, encrinites, &c.? Beautiful specimens of the polished encrinal limestone have been spread abroad from Lockport.

The corrections already made are not more numerous or important, than were to be expected from the state of geological knowledge when the survey had been in considerable part made.

Murchison's work on the Silurian system had not appeared, and the field was new and to a great extent unexplored. After all that had been accomplished by Prof. Eaton in his survey of the strata along the Great Western Canal, and by others, the exploration, as connected with general geology, could scarcely be said to have been begun. Probably others yet remain to be accomplished. Too many groups may have been formed. While *Cryptolithus tesselatus* retains its place with some others in the Llandeilo flags and in the lower part of the Trenton limestone, and in that of the Mohawk at Fonda and in other places, that rock of Wales may yet find its equivalent in our series. The greater number and variety of our groups may not involve all those described by Murchison, but only the fullest examination should be satisfactory.

Finally. The *discoveries* which have been made by the paleontologist. These are many, and of great interest. Omitting all the organic remains which have been identified with the European, and the many new genera and species, there are several general facts of great value.

First: The identifying of the sandstone of Blossburg, Penn. with the old red sandstone of Europe, a part of the Devonian system. The geological positions of the two rocks are very nearly or quite the same; and the remains of the fish, *Holoptychus nobilissimus*, found before only in the old red sandstone, seem to settle the point conclusively. The stumbling block of our geology is thus removed. The Blossburg sandstone is the upper part of the Devonian system. The Chemung group, as it is called in the geological reports, forms the lower part of the Devonian, and is characterized by the same shells. pp. 42 and 43.

Second: The carboniferous system in Pennsylvania and Ohio, contains many species of shells found in the same system in Europe. Even "at Engineer Cantonment, Missouri River," the shells of the carboniferous system are found, as well as at Pittsburgh and on the Alleghany Mountains. Eleven species are named as common to the carboniferous strata in our country and Europe. p. 43.

Third: "Well characterized and undoubted oolite, in the state of Ohio," is *for the first time* announced in this report. p. 44. "Two European species of *Trigonia*, both of which are restricted to the oolitic system," are presented as the proof. It has

long been known that the oolite of Saratoga County could not belong to the oolitic series of Europe.

Fourth: Eleven species of organic remains are given to identify the lower cretaceous series of New Jersey, Delaware, and the Atlantic states further south, with that of Europe. p. 45. Mr. Conrad discovered the middle division of this series at Wilmington, N. C. The upper division is stated to exist in South Carolina and Georgia, and to abound "in the southern counties of Alabama and in Florida." p. 45.

Fifth: The "lower tertiary" was *first noticed* by Mr. Conrad, and shown to be the same with the "Eocene formation." "In Georgia, and more rarely, in Alabama, a portion of the formation assumes the character of burr stone," containing beautiful shells finely "silicified," which we admired years ago, and were compelled to separate the rock from the Paris burr-stone, which is destitute of shells. On the Potomac, at Fort Washington, is the same lower tertiary, and at Claiborne, Alabama, Mr. C. found in it "about two hundred species of shells and corallines, many of which are identical with the Eocene species of Europe." p. 46.

Sixth: The rocks of the older Silurian system, terminating upwards with the *Pentamerus oblongus limestone* in this report, seem to be bounded on the south by the Mohawk River and the Erie Canal nearly, and which are covered by the mountain ridge at Lockport and Niagara River, &c., *reappear* "at Bedford Springs, and in the vicinity of Cincinnati, Ohio," according to the discovered fossils. p. 32. Indeed, the organic remains at the west seem likely to identify more of the rocks. What an uplifting of the strata in Ohio, must have taken place, or a mighty wearing away of the incumbent series which extend westward under the waters of Lake Erie, or a cutting off of the Silurian of this State from that of Ohio by the ridge through which the Niagara River passes.

In conclusion, reference might be made to the unnamed fucoids of the Niagara sandstone, which are abundant below the lower falls of Genessee, and the numerous shells and fucoids above this sandstone, which remain to be identified or to increase the number of the species. Standing as we do on these remains, the coming and final report is expected to reveal a world of mysteries and settle a host of difficulties.

At Lockport, is found a coralline, probably a *Porites*, which for distinction's sake, I will call *P. gypsea.* It occurs alone, or with laminated gypsum, often clear and fine selenite, diffused through it; or rather, the petrifaction of carbonate of lime has been converted into the sulphate, and crystallized where it was formed. Sometimes this *Porites* is divided, *as if by a saw*, into separate portions, straight; and the sections at various inclinations to each other. The cuttings are narrow, sometimes a fourth of an inch wide. The gypsum still lies in many of these apparent straight cuttings or saw-cuts; in others the gypsum has been dissolved, and the cuts are empty. Some of the specimens have great beauty. It is evident that the petrifaction has undergone this operation. How should it be divided by these straight cuts? Could the sulphuric acid be generated in that place, and for any reason follow a course which appears to have been drawn by a rule?

ART. III.—*On the Manipulations of the Dipping Compass;* by JOHN LOCKE, M. D., Prof. of Chem. in the Med. Coll. of Ohio, and Lecturer on Nat. Philos. in the Ohio Mechanics' Institute.

Messrs. Silliman—Every practical magnetician is aware of the great difficulty in obtaining consistent results, with even the most improved dipping apparatus. In my late communications to your Journal, it appears that I have succeeded, with an apparatus made by Robinson of London, so far that the discrepancies between the results of the two needles used, seldom amount to one minute of a degree. In the same communications I alluded to some peculiar manipulations which I adopted first at Davenport in Iowa, in September of 1839, and to which I attributed the superior consistency of my subsequent results. Having been honored with a verbal request by several distinguished collaborators, that I would communicate my views on this subject, I am induced to lay before your readers the following remarks.

In the instrument used by me, the dipping needle moves on small pivots supported by straight polished agate edges, on which they roll like a wheel upon a rail. As such a motion is not only rotary but progressive, the centre of the needle is carried out of

the centre of the graduated circle, by the same rotation which brings it to the true dip. To restore the pivots to their place, two Vs and two inclined planes* raised by a lever engage them at their sides and at their ends, adjust them laterally and longitudinally, raise them just clear of the agate edges, and, by a reverse motion, lower and deposit them, and with them the needle in their proper places. Now the most objectionable anomaly has been, that when the same needle has been once adjusted by the Vs, read, and again simply raised by the Vs, and lowered *apparently in the same place*, and read again, there would be a discordance in the readings, often amounting to five minutes of a degree. I felt the full weight of these anomalies in my first attempts to determine the dip, and found to my mortification, that the mean results of the usual eight reversals and sixteen readings, with two different needles, would sometimes differ as much as six minutes of a degree.

I finally reflected that if the pivots could always be made to re-sit on *exactly the same* points, the readings must always agree, and that the apparent anomalies must arise from slight imperfections of form, and slight and imperceptible, but really essential, changes of position of the points of support to the pivots. With this view I endeavored by all possible means, so to use the apparatus as to bring the bearings at the same points, and especially that the pivots should perform their rotations on the same transverse section; or, in other words, that the same ring or circle of their circumference, should always rest on the agate edges. To accomplish this, the following points received attention.

1. The Vs and inclined planes, which raise and fix the needle to its place, were so adjusted as to allow of no shake or lateral motion of their own.†

2. The distance between each agate edge and the inclined plane, opposed to the end of the pivot operating as an end check, was made as nearly equal as possible.

3. Although the pivots had some "end chase" or longitudinal freedom between the end checks yet in use, each pivot was always slid over against one particular end check by means of a

* These inclined planes slope up opposite the ends of the pivots, and are really two parts of a V widely separated.

† I have lately examined one of Gambey's instruments, which was decidedly faulty in this particular.

slight pressure with the pencil against the side of the needle; say, when the face of the needle is westward the pivot is shoved over till it is checked on the west side, and when the face of the needle is eastward it is drawn over until checked on the eastern side.

4. When the needle has been placed in the compass and its reading noted, it should not be immediately taken out and reversed, but be suffered to remain exactly on its bearings, and the compass be *carefully* reversed in azimuth, and with it, the needle, when the second reading should be made, after which the needle itself may be taken out and reversed. The reversals and readings will then have the following order:

1. Face of the compass east, and face of the needle east.
2. Face of the compass west, and face of the needle west.
3. Face of the compass west, and face of the needle east.
4. Face of the compass east, and face of the needle west.

In the above, it will be seen that although the needle in relation to the face of the instrument and to the "points of compass" assumes four positions, yet to attain them it is removed from its place in the Vs but once, and that is between the second and third reading.

I am aware that it may be objected to all of this, that although I may obtain consistent results, still they may not indicate the true dip, for if the pivots of each needle be alike, although not perfectly cylindrical, and alike placed with reference to the axis of the needle, the mean results would agree to the same error. To this objection we observe first, that it is by no means probable that two pivots intended to be cylindrical should both assume another form, and both be exactly alike, and that these forms, even supposing them to be alike, should be alike placed in reference to the needle's axis; and secondly, we would remark that although we may involve an error by our consistent mode of observation, it would be a constant one, both in quantity and kind, might be ascertained by a great number of comparisons with the results of other instruments, or by other obvious means, and be corrected by a constant equation.

As an example of the order of reversals observed by me, I give below the result of my observations at Baltimore, April 28, 1841.

At the first square northeast of the Washington Monument, called Howard's Woods. 6h. 37m. to 7h. 47m. A. M.

Needle No. 1. A North.			Needle No. 2. A North.		
E.	E.	70°38′.5	E.	E.	71°37′.5
W.	W.	73 03.5	W.	W.	71 16
W.	E.	70 35.5	W.	E.	71 30.5
E.	W.	73 05.0	E.	W.	71 08
B North.			B North.		
E.	E.	72 52	E.	E.	71 55
W.	W.	69 53	W.	W.	71 35
W.	E.	72 36	W.	E.	71 52
E.	W.	69 56	E.	W.	71 34.5
		8)572 39.5			8)572 29.5
		Mean=71 34.94			Mean=71 33.7
					Mean of No. 1=71 34.94
					2)143 08.64
					Mean total=71 34.32

At the Botanical Garden of St. Mary's College, Baltimore, April 28, 1841. 12h. 50m. to 1h. 10m. P. M.

Needle No. 1. A North.			Needle No. 2. A North.		
E.	E.	70°35′.5	E.	E.	71°53′.5
W.	W.	73 00	W.	W.	71 44.5
W.	E.	70 35.5	W.	E.	71 48.5
E.	W.	73 07	E.	W.	71 45.5
B North.			B North.		
E.	E.	72 46	E.	E.	71 37
W.	W.	70 20	W.	W.	71 32.5
W.	E.	72 38.5	W.	E.	71 32.5
E.	W.	70 14	E.	W.	71 16.5
		8)573 16.5			8)573 10.5
		Mean=71 39.56			Mean=71 38.81
					Mean of No. 1=71 39.56
					2)143 18.37
					Mean total=71 39.18

At both localities Mr. Nicollet made observations on the same day and nearly at the same hours with a similar apparatus, manipulating at my request in the same manner as I have just described. The results were remarkably coincident, as have been others at the same localities.

At Howard's Woods.		At St. Mary's College.	
By Prof. Bache, Aug. 28, 1840,	71°34′.4	Prof. Bache did not observe at this locality.	
By Prof. Locke, April 28, 1841,	71 34.3	By Prof. Locke, April 28, 1841,	71°39′.2
By Mr. Nicollet, April 28, 1841,	71 34.9	By Mr. Nicollet, April 28, 1841,	71 38.6
By Maj. Graham, June 10, 1841,	71 31.9	By Maj. Graham, June 11, 1841,	71 38.8

ART. IV.—*The Involution of Polynomials;* by WM. J. LEWIS, Civil Engineer, Germantown, Penn.

IF any binomial $a+b$ be raised by actual multiplication to the 2d, 3d, 4th, 5th, and nth powers, we find that the first and second terms of the powers are a^2+2ab, a^3+3a^2b, a^4+4a^3b, a^5+5a^4b, and $a^n+na^{n-1}b$ respectively.

Let it be required to raise to the 5th power any expression $a+b+c+d+e$, consisting of at least five terms.

Then considering $b+c+d+e$ first as one term, then as made up of $b+\overline{c+d+e}$, and subsequently regarding $\overline{d+e}$ as one term; and retaining only the second term of the first involution, and the first and second of the others, we have

$$\overline{a+b+c+d+e}^5$$
$$=5\overline{a+b+c+d}^4e+\&c. \quad (=5a^4e+\&c.)$$
$$=5.4\overline{a+b+c}^3de+\&c. \quad (=5\cdot4a^3de+\&c.)$$
$$=5\cdot4\cdot3\overline{a+b}^2cde+\&c. \quad (=5\cdot4\cdot3a^2cde+\&c.)$$
$$=5\cdot4\cdot3\cdot2abcde+\&c. \quad (=5\cdot4\cdot3\cdot2abcde+\&c.)$$

Hence, if P=coefficient of $abcde$, then will $\frac{P}{2}$ = coefficient of a^3bcd, $\frac{P}{2\cdot3}$=coeff. of a^3bc, $\frac{P}{2\cdot3\cdot4}$=coeff. of a^4b, and $\frac{P}{2\cdot3\cdot4\cdot5}$= coeff. of a^5.

If our root had consisted of more than five terms, P would have represented the coefficient of the product of any five terms, $\frac{P}{2}$ the coeff. of the product of a^2, and three other terms, $\frac{P}{2\cdot3}$ the coeff. of a^3 multiplied by any two other terms, &c.

The coefficient of a^3bcd is also the coefficient of ab^3cd, abc^3d, $abcd^3$, &c.; the coefficient of a^3bc is the coefficient of ab^3c, abc^3, abd^3, &c.; and generally, any term can be substituted for a in the above expressions. For either of the terms b, c, d, e, can be placed first in the root, when it will be subject to the same operations as have been performed on a, and will consequently be substituted for it. Our remarks, therefore, in relation to the powers and coefficients of a are equally applicable to the powers and coefficients of all the other terms.

We see that when the powers of a are connected with the product of other terms, a is changed into a^2 by dividing the coefficient of a by 2, a^2 into a^3 by dividing its coefficient by 3, &c. That is, *the power of any term may be increased one by dividing its coefficient by the index of the power to which it is to be raised.*

And conversely, *the power of any term may be depressed one by multiplying its coefficient by the index of its power.*

We have not yet ascertained whether the law may be extended to those terms in which the powers of two or more terms of the root are combined, as a^2c^2de, a^2c^3, &c.

Let N be the coefficient of a^2c^2d, and put $c=m+n$, then $Na^2c^2d=Na^2d\overline{m+n}^2=2Na^2dmn+$&c. Hence, $N=\frac{1}{2}$ coeff. of $a^2dmn=\frac{1}{2}$ coeff. of a^2cde. Therefore, coeff. of $a^2c^2d=\frac{1}{2}$ coeff. of a^2cde.

Again, let m be the coefficient of a^2c^3, and putting $c=m+n$, we have $Ma^2c^3=Ma^2\overline{m+n}^3=3Ma^2m^2n+$&c. Hence, $M=\frac{1}{3}$ coeff. of $a^2m^2n=\frac{1}{3}$ coeff. of a^2c^2d.

Therefore the coeff. of $a^2c^3=\frac{1}{3}$ coeff. of $a^2c^2d=\frac{1}{2.3}$ coeff. of $a^2cde=\frac{1}{2\cdot2\cdot3}$ coeff. of $abcde=\frac{P}{2\cdot2\cdot3}$. A similar process applied to any combination of the powers of the terms of the root, will evidently show, that the coefficients are governed by the law which has been given. We remark then:

1. That P, the coefficient of the product of as many terms of the root as there are units in n (the index of the power) $=n\ \overline{n-1}\ \overline{n-2}\text{-----}3\cdot2\cdot1$.

2. That the coefficients of terms involving the powers of one letter and the product of others are obtained by dividing P by 2, this quotient by 3, this again by 4, &c., the last divisor being $n-1$, and the final quotient n.

3. That the coefficients of terms combining powers of two or more terms are obtained by dividing these results by 2, 3, 4---- $n-2$ successively.

4. That the coefficient of any term $a^mb^rcd=\frac{P}{1,2,3\text{-----}m\times1,2,3\text{-----}r}=\frac{n\ \overline{n-1}\ \overline{n-2}\text{----}\overline{m+1}}{1,2,3\text{-----}r}$, and that the coefficient of $a^mbcd=n\ \overline{n-1}\ \overline{n-2}\text{-----}\overline{m+1}$. If the root contains only a and b, or c, d, e, &c. are each $=0$, then all the

terms containing these letters disappear from the equation, and we have $\overline{a+b}^n = a^n + na^{n-1}b + n\frac{n-1}{2}a^{n-2}b^2 + n\frac{n-1}{2}\frac{n-2}{3}a^{n-3}b^3 +$&c. in which n, $n-1$, $n-2$, &c. arise from the decrease of the powers of a, and the denominators from the increase of the powers of b. This is the well known Binomial Theorem.

Put $\overline{a+b}^n = a^n + na^{n-1}b + Aa^{n-2}b^2 + Ba^{n-3}b^3 + Ca^{n-4}b^4 +$&c.

If a third term c is introduced, we shall have the following additional terms:

$$2Aa^{n-2}bc + 3Ba^{n-3}b^2c + 4Ca^{n-4}b^3c + 5Da^{n-5}b^4c +\text{&c.}$$

If a fourth term d is now introduced, we shall have again as additional terms:

$$2\cdot3Ba^{n-3}bcd + 3\cdot4Ca^{n-4}b^2cd + 4\cdot5Da^{n-5}b^3cd +\text{&c.}$$

If a fifth term e, we must again add,

$$2\cdot3\cdot4Ca^{n-4}bcde + 3\cdot4\cdot5Da^{n-5}b^2cde +\text{&c.}$$

We must not forget that there are several terms in the expression for the power, involving like powers of different letters, (as a^4b^3cd, a^3b^4cd, ab^3c^4d, and abc^2d^4,) and having like coefficients; but only one of each of these terms has been given; this being sufficient to indicate the magnitude of all the coefficients.

When n is large and the number of terms in the root is small, it is most convenient to find the coefficients of a binomial, and afterwards obtain from these the additional coefficients for the other terms, as shown in the last process. In many cases, however, it is better to find the higher coefficients first.

Example 1. Find the coefficients of $\overline{a+b+c}^3$. Here we have $P=3\times2=6$, $\frac{P}{2}=3$, and the form of the power is $\overline{a+b+c}^3 = a^3+3a^2bc+6abc+$&c.

Ex. 2. Find the coefficients of $\overline{a+b+c+d}^4$.

Here $P=4\times3\times2=$ - - 24	$abcd$
$\frac{P}{2}=$ - - - - - - 12	a^2bc
$\frac{P}{2\cdot2}=$ - - - - - 6	a^2b^2
$\frac{P}{2\cdot3}=$ - - - - - 4	a^3b
$\frac{P}{2\cdot3\cdot4}=$ - - - - - 1	a^4

Hence $\overline{a+b+c+d}^4 = a^4+4a^3b+6a^2b^2+12a^2bc+24abcd+$&c.

Ex. 3. Find the coefficients of $\overline{a+b+c+d+e}^5$.

Here $P=5\times4\times3\times2\times1=120$	$abcde$
$\frac{P}{2}=$ - - - 60	a^2bcd
$\frac{P}{2\cdot2}=$ - - - 30	a^2b^2c
$\frac{P}{2\cdot3}=$ - - - 20	a^3bc
$\frac{P}{2\cdot3\cdot2}=$ - - - 10	a^3b^2
$\frac{P}{2\cdot3\cdot4}=$ - - - 5	a^4b
$\frac{P}{2\cdot3\cdot4\cdot5}=$ - - 1	a^5

Hence $\overline{a+b+c+d+e}^5=a^5+5a^4b+10a^3b^2+20a^3bc+30a^2b^2c+60a^2bcd+120abcde+$&c.

If the number of terms in the root is greater than the index of the power, the excess produces no change in the coefficients, as no more than n letters can enter any term of the power.

Ex. 4. Find the coefficients of $\overline{a+b+c+d}^6$.

The binomial coefficients are 1, 6, 15, 20. The introduction of the third term c, gives us $2A=30$, $3B=60$. And the fourth term d, gives $2\cdot3B=120$, $3\cdot4C=180$.

Hence $\overline{a+b+c+d}^6=a^6+6a^5b+15a^4b^2+20a^3b^3+30a^4bc+60a^3b^2c+120a^3bcd+180a^2b^2cd+$&c.

Or we might have found P as before, observing that as two terms are deficient in the root, P must have at least two divisions before the consequent coefficient can enter into the expression of the power.

$P=6\times5\times4\times3\times2=720$	$abcdef$
$\frac{P}{2}=$ - - - 360	a^2bcde
$\frac{P}{2\cdot2}=$ - - - 180	a^2b^2cd
$\frac{P}{2\cdot3}=$ - - - 120	a^3bcd
$\frac{P}{2\cdot2\cdot3}=$ - - 60	a^3b^2c

$\frac{P}{2\cdot3\cdot4}$ = - -	30	a^1bc
$\frac{P}{2\cdot3\cdot4\cdot2}$ = - -	15	a^1b^2
$\frac{P}{2\cdot3\cdot4\cdot5}$ = - -	6	a^3b

But $e=0$, and $f=0$. Hence, $\overline{a+b+c+d}^6 = a^6 + 6a^5b + 15a^4b^2 + 30a^4bc + 60a^3b^2c + 120a^3bcd + 180a^2b^2cd +$ &c.

ART. V.—*Notice of a Hurricane that passed over New England in September*, 1815; by NOYES DARLING, Esq.

1. SOME circumstances attending this remarkable storm, induced me at the time, to make a collection and abstract of all the newspaper accounts of it which I could find. I was enabled from my situation, then in New York, to make the collection sufficiently ample to present a pretty full view of the storm in the greater part of its extent. Believing that the fruit of my labors may interest and perhaps be useful to those who are engaged in the investigation of "Atlantic hurricanes," I am induced to offer it for publication.

1. *Accounts of the Storm at Sea.*

2. Lat. 17° 54′ N., lon. 63° 10′ W., Sept. 18. *Schr. Phœnix, St. Barts.* Violent gale at that island on the 18th, which lasted thirty hours, from N. W.—W. and S. Forty vessels driven ashore.

3. Lat. 21° 18′, lon. 71° 5′, Sept. 20. *Ship William, Turks Island.* Violent hurricane at that island on the 20th from N. E. to S. W.; unroofed and blew down houses, &c. Lasted from morning to 4 P. M.

4. Lat. 32°, lon. 74° 50′, Sept. 22. *Schr. Return*, experienced a tremendous gale from S. E., which compelled us to cut away foremast. About 4 P. M., very heavy sea struck her and carried away bowsprit, bulwarks, &c. If the gale had not abated, she must have gone down. Another account.—On Friday, 22d, Schr. was in lat. 33°, lon. 74° 55′. At 6 A. M., a gale commenced at S. E., which continued with great violence till 7 in the evening. At 3 P. M. cut away foremast.

5. Lat. 32° 25′, lon. 70° 10′, Sept. 22. *Sloop Experiment*, on the morning of the 22d was upset, in a heavy gale from N. W.

which lasted eight hours, and remained on beam ends twenty two hours and then righted. Captain and mate taken off the wreck by Schr. Nelson, on the 24th, in lat. 38° 2′, lon. 75° 15′.

6. Lat. 33°, lon. 74°, Sept. 22. *Schr. Rover*, in a terrible gale on the 22d, lost main-mast and most of the canvass.

7. Lat. 33° 10′, Sept. 23. *Brig Sarah*, from St. Pierre's to New London; a very heavy gale and tremendous sea from S. E. to S. S. W.

8. Lat. 34° 20′, lon. 70° 50′, Sept. 23. *Schr. Indian Queen*, experienced a tremendous hurricane about 12 at night from E., and knocked on her beam ends. Gale lasted about four hours. Another account says five hours.

9. Lat. 34° 21′, lon. 71° 37′, Sept. 23. *Brig George*, experienced a tremendous gale from S. E., which lasted twelve hours.

10. Off Cape Hatteras, Sept. 22d, *Ship Minerva*, in fifteen fathoms water, encountered a tremendous gale from S. E. to N. W., main topmast carried away. At 3 A. M. wind shifted to northward, and became more moderate.—Sept. 22d, *Ship Phœnix*, experienced a most violent gale, which commenced at S. E. and ended in four hours at E. S. E. Lost topmasts, yards, &c.—*Schr. Ruby*, capsized on 22d.

11. Lat. 36° 30′, lon. 74°, Sept. 22. *Brig Morgiana*, experienced a heavy gale, which swept the decks.

12. Lat. 36° 44′, lon. 73° 17′, Sept. 22. *Schr. Thetis*, experienced a heavy gale from N. N. E. Lost fore and main topmast, &c. 23d, night; *Schr. Spartan*, from Marseilles to Baltimore. Sept. 2d, was in lat. 35° 58′, lon. 38°. On the 24th, was in lat. 37° 32′, lon. 72° 14′. On the 23d in the night experienced a very heavy gale from S. E. and S.

13. Lat. 37° 30′, lon. 72°, Sept. 23. *Brig Statira*, in the Gulf Stream, experienced a violent gale which carried away mainmast, yards, sails, &c. On her beam ends a considerable time after the gale. Another account.—On the 23d commenced with strong gales from S. E., close reefed topsails, &c.; at 3 P. M., took in fore topsails; at 4, took in main topsail, gale increasing hove to; at A. M. brought her more head to the wind; at 1 A. M., balance reefed the topsail; at 2, deck load shifting, cut away mainmast and she fell before the wind; at 3 sea swept the deck; at 10 gale abated, sea continued very high and irregular, being in Gulf Stream. Wind now shifted to W. S. W. as judged, for the com-

pass would not stand at any point during the gale. *Schr. Merino*, from Port au Prince to Boston, was in lat. 37° 18′, lon. 74°, on the 24th; experienced a heavy gale, carried away deck load and bowsprit.

14. Off the Capes, Sept. 23. *Schr. Traverse the Ocean*, for Baltimore, took the gale of the 23d, and was driven a long way S.; split her sails, &c.

15. Lat. 37°, lon. 76°, Sept. 23. *Schr. Sally*, experienced a heavy gale from E. N. E. to W., which lasted seven hours.

16. Off the Capes of Delaware, Sept. 23, night. *Brig Polly*, nearly on soundings, experienced a severe gale from S. W., cut away foremast, lost sails, &c.

17. Off Barnegat. *Schr. Alexander.* A severe gale commenced from E. N. E., which continued till 7 A. M., when it calmed and directly afterwards came from W. N. W. with great violence; stove in bulwarks. Never experienced a more violent gale. *Schr. Fair American*, for Alexandria, on the 22d was in lat. 39° 36′, lon. 70° 40′. Next day experienced a tremendous hurricane.

18. Lat. 39° 45′, lon. 72° 17′, Sept. 23. *Brig Connecticut*, experienced a gale in which she lost her bowsprit, &c. *Brig Amigo*, for New York, was in lat. 39°, lon. 71° 30′ on the 24th. Had a violent gale for four hours on the 23d.

19. Lat. 40°, Sept. 23. *Brig Othello*, for New York, in forty fathoms water; gale beginning from E. S. E. and veering round to W. S. W., blowing a storm from 4 A. M. to noon. Coming into New York the 26th. *Brig Morgiana*, Sandy Hook, W. N. W. twenty leagues; upset about half past eight in a violent hurricane.

20. Lat. 40°, lon. 72° 50′, Sept. 23. *Brig Henrico*, experienced the gale most violently. *Ship Balloon*, from Amsterdam to Philadelphia, was in lat. 40°, lon. 72° 3′, on the 24th. Experienced a severe hurricane from S. E. to S. W. on the 23d; lost main and fore topmasts. Another account.—Sept. 21, lat. 41°, lon. 64°. Sept. 24, lat. 39°, lon. 72°: severe hurricane on the 23d, from S. E. to S. W.

21. Lat. 40° 10′, lon. 70°, Sept. 22. *Schr. Rising States*, experienced a heavy gale from E. N. E. to W. S. W.; heavy cross sea running. *Brig Abaellino*, fifteen leagues westward of south shoals of Nantucket. Severe gale—carried away bulwarks.

22. Lat. 41° 41′, lon. 60°, Sept. 22d. *Two vessels* spoke, but no mention of the gale.

23. Lat. 41° 51′, lon. 63° 45′, Sept. 22. *Ship Mandarin*, on the 22d, experienced a heavy rolling sea, but little wind; then about one hundred and fifty miles S. E. of Boston.

24. Lat. 42° 28′, lon. 66°, Sept. 23. *Ship Thomas*, wind from N. W. to E. and S.; part of the day moderate, and part fresh breezes.

25. Off Cape Ann, Sept. 23. *Schr. Two Sisters*, sixteen leagues off Cape Ann—felt nothing of the gale.

26. East of Cape Ann, Sept. 23. *Schr. Leopard*, when five leagues E. of Cape Ann; experienced the gale very severely—thrown on beam ends. *Brig Caroline*, two hundred miles N. E. of Boston, a fresh breeze from N. E. to S.

27. Sept. 23. *Ship Prudence*, twenty leagues S. E. from St. George's shoals, had a tremendous swell from S. W. and lay to under reefed mizzen stay-sail, expecting a gale, but had nothing more than a balanced reef breeze; at midnight, set balanced reefs again, with strong westerly winds.

28. Sept. 24. *Brig Fredonia*, on 23d in lat. 36° 51′, lon. 73° 20′, and on 25th, Cape Henlopen, W. N. W. fifty miles; on 24th, a tremendous gale commenced at E. N. E. but shifted to N. W. *Schr. Gov. Shelby*, from Bordeaux to New York, arrived October 5th. Sept. 18th, was in lat. 39° 40′, lon. 43° 30′; experienced the gale within two days' sail of port, but received no injury. Since then, there has not been at sea a one knot breeze. *Schr. Comet*, from St. Barts to Baltimore: in the edge of soundings experienced a heavy gale; lay to ten hours—25th, took a pilot; 26th, came into bay.

2. *Accounts of the Storm on Land.*

29. *Philadelphia.* Great part of Friday night (22d) wind, a gale from N. E. with heavy rain. Early Saturday (23d) veered to N. W., and continued a gale, with torrents of rain, for several hours. Between 8 and 9 o'clock wind slackened, rain ceased, and clouds broke away in W. and S. W. About noon weather clear and mild, with a gentle westerly breeze. During the afternoon the sun greater part of the time obscured with flying clouds from W. and N. W.

30. *New York.* Thursday night? (21st) violent storm of wind and rain set in from N. E. and continued till about 2 o'clock at night, when it suddenly shifted to N. and N. W., and blew with

increased violence. Friday (22d) gale all day from N. E. and E., with heavy and incessant rain. Gale increased in the evening, continued till 4 o'clock, Saturday. At 2 o'clock in the morning backed round to N., and by 9 o'clock was at N. N. W., when it was most violent. In the course of the forenoon gradually backed round to S. W.

31. *Bridgeport, Ct.* Account is lost, but I find in a table which is subjoined, the following :—Wind N. E. at 6, 7, 8, 9, 10, ½ past 10, and N. W. at 11 o'clock, of 23d.

32. *New Haven, Ct.* Friday night and Saturday morning (22d and 23d) severe storm of wind and rain. Did damage to roads and bridges, wharf inundated; six and a half inches of rain fell during storm; streams much swelled. Wind N. E. from morning of 22d to morning of 23d; noon of 23d W., evening S. W.

33. *Martha's Vineyard, Mass.* Gale very severe.

34. *Lyme, Ct.* Account lost, the following is from the table :— Wind N. E. at 6 and 7 o'clock, S. E. at 8 and 9 o'clock of 23d.

35. *New London, Ct.* Storm commenced on Friday (22d.) During that day and night a heavy fall of rain, wind N. E. Next morning (23d) wind increased, at 7 o'clock very violent, soon after almost a hurricane. The tide which commenced flood about 6 o'clock, covered the wharves before 9, and at 10 o'clock had risen three or four feet higher than was ever known. The rise had been so rapid, that the buildings in Beech street were deluged before the inhabitants felt their danger, and in thirty minutes after danger was apprehended waves rose four to six feet in the streets. Now stores were falling, buildings unroofed, trees falling; this destructive scene was short. Soon after 11 o'clock the wind shifted to the westward and abated, when the sea returned with the velocity it came in, though it should have run flood till 12; and the storm ceased. The destruction of trees in all towns in the neighborhood was immense. Intelligent farmers estimate half the best fruit and forest trees fallen. The showers which fell over this city and neighborhood were of salt water. The leaves of tender fruit trees and shrubs and of many forest trees, without frost, shrunk in a few hours after the gale as though they had been scorched. During the strength of the wind, in the eddies, the air was extremely hot and suffocating.—Another account. For two or three days wind blew from N. E., not very hard; about 8 o'clock it shifted to E., when its severity commenced.

Between 9 and 10 o'clock veered to S. E., when it blew most violent.—October 4th. The brooks which run through this place continue brackish. Some wells in the country have become brackish.

36. *Stonington, Ct.* Storm raged with great violence. Every vessel went ashore. Thirty buildings were destroyed or injured. The following is from the table:—Wind N. E. at 6, 7, 8, and 9 o'clock, and S. E. at 10 o'clock.

37. *Newport, R. I.* Commenced about 9 o'clock, wind S. E. by S., and continued unabated two hours and a half, when it subsided.

38. *Norwich, Ct.* Wind during most of the week blew moderately from the east with pleasant weather until Thursday, (21st,) when it became cloudy and uncommonly raw and cold. Friday morning (22d) it began to rain and continued the whole day. At night blew fresh from N. E., gradually increasing till about 8 o'clock, Saturday morning (23d) when it veered to E. S. E., and blew with tremendous fury from that point to W. S. W., till near 12 o'clock, when it abated. Many trees were levelled.

39. *Fair Haven, Mass.* Morning, wind blew from S. E., very hard. About 9 o'clock shifted to S. and remained two hours a tremendous gale. About 12 o'clock was S. W. and continued so the rest of the day, blowing hard with heavy rain most of the time. Windows covered with salt water; trees turned black.

40. *New Bedford, Mass.* Account lost; the following is from the table:—Wind S. E. at 6 o'clock and continued there till 12.

41. *East Greenwich, R. I.* Gale commenced about 7 o'clock and continued till 12. Tide rose seven feet higher than was ever known. Meeting-house unroofed.

42. *Warren, R. I.* Tide rose about seven feet above common spring tide. Trees, buildings, &c. demolished.

43. *Providence, R. I.* At 7 o'clock, wind shifted from N. E. to S. E., at which point it seemed to be settled in the course of half an hour. At 8 o'clock, from being cold, the air became suddenly very warm; so much so, that standing by a window looking eastward, sensations were felt not unlike standing before an oven moderately heated. At 9 o'clock, scuds run very low; the sky when visible looked very glassy, something like brass. The atmosphere seemed very much impregnated with saline particles, quite perceptible to the taste. At 9½ o'clock it blew a gale, and

continued to increase in violence till 11, when the wind shifted to S.; the tornado then began to abate. At 12 o'clock, wind veered to S. W. by S., when storm ceased. Another account:— A storm of rain from N. E. commenced on Friday, (22d,) and continued with little intermission till Saturday morning, (23d,) when wind was from E. Between 8 and 9 o'clock wind shifted S. E., and continued to blow, increasing in violence, till 11½, when it changed to W., and damage stayed.

44. *Poughkeepsie, N. Y.* Gale, but little or no damage done.

45. *Worcester, Mass.* Thursday [Friday?] evening (21st) [22d?] heavy storm (rain) commenced, with strong N. E. wind, which had been blowing twenty four hours before from that quarter. Early Saturday morning (23d) the wind increased, and rain descended in torrents, and continued with but short intermissions until about 10½ o'clock, when the rain abated and the wind suddenly shifted to S. E. and blew a hurricane, blowing down trees and chimneys. We have traced a column of near sixty miles in width, with nearly the same devastation. No parallel in this country. Period of destruction about one hour. Wind came in gusts with increasing violence until its utmost height, when it gradually subsided to a gentle breeze. A suffocating current of air, as from a hot bath, accompanied the middle stage of the tempest. The destruction of forest trees incalculable. Grapes in a garden had a taste of salt on their surface. Flocks of gulls were seen after the storm on Saturday, in a meadow near Worcester, and others about the same time in Grafton. Toward evening they flew toward the sea. Water which fell in Uxbridge, Grafton, Worcester and Sterling, salt.

46. *Boston, Mass.* Storm of rain from N. E. commenced on Friday, (22d); through the day moderate; at night rain increased, and wind somewhat violent. During the night it abated. Saturday morning storm renewed its violence. Wind with accumulating severity from E. till near 11 o'clock. At this time shifted to S. E. but increased in violence until 12 o'clock, when it began to abate, and between 1 and 2 o'clock shifted to S. W. At 2 o'clock danger from wind over, and at the close of the afternoon it had entirely subsided. About 12 o'clock, two hours before high water, when the gale from the S. E. was at its height, the tide rose very high. After change of wind it did not continue to rise; the wind compelled a fall earlier than natural. Glass-house

blew down about 11 o'clock; trees were thrown down. Every substance exposed to rain incrusted with salt; windows lost their transparency from the salt; leaves have the appearance of frost. Same observed some miles interior. Another account:—Storm commenced at 4 o'clock from E., with heavy showers. At 9 o'clock fresh gale from E. with slight rain; at ¼ before 11 shifted to S. E. without rain, and by 12 o'clock a violent hurricane. At 2 o'clock gale had abated; and at 6 o'clock moderate weather.

47. *Salem, Mass.* Saturday, (23d,) violent gale began about 9 o'clock, greatest fury about 11½ o'clock. Began at S. E. and filled the air with rain as briny as the ocean. At the time of greatest violence changed to S. W., and then did greatest damage; it afterwards changed to S. S. W., and before 3 o'clock the sun appeared. Not suffered as much as our neighbors. Loss of barns, out-houses, orchards and fences severely felt. Few vessels injured. Another account:—Morning, wind at E., about 11 shifted to S. E.; between 1 and 2 o'clock got S. W., and soon subsided; pleasant before night.

48. *Northampton, Mass.* Storm very severe only on Saturday (23d).

49. *Amherst, Mass.* All the country within this place, Brookfield, Tolland, New London, New Bedford, tempest raged about equally. Great destruction of trees. The following is from the table:—wind at 11½ o'clock S. E.; at 1½ o'clock subsided.

50. *Provincetown*, and *Wellfleet, Cape Cod.* The gale was but slightly felt.

51. *Troy, N. Y.* Great rain, sudden and unusual rise in Hudson River. Sunday, (24th,) most of the wharves covered several feet deep.

52. *Portsmouth, N. H.* Buildings considerably injured.

53. *Counties of Bristol, Barnstable, Plymouth, Norfolk, Worcester, Middlesex, Essex* and *Hampshire, Mass.* Reports that would fill columns. Damages pretty equally felt in injury of meeting-houses, dwellings, chimneys, barns, and trees. All fruit shaken off. In all places to leeward of salt water, pastures ruined by the salt spray, and the whole of trees and vegetables so blighted and changed as to exhibit the appearance of destruction by fire and smoke.

3. *Accounts showing the force of the wind in several parts of Massachusetts.*

54. *Abingdon.* Church destroyed.—*South Reading.* Steeple blown down.—*Wareham.* Steeple blown down.—*Cambridgeport.* Three dwellings demolished.—*Dorchester.* Seventeen houses unroofed, sixty chimneys blown over, and five thousand trees prostrated.—*Cape Towns.* No accounts of severe damage except at *Sandwich.*—*Chelsea.* Elm tree seventeen feet in girth, blown down.—*Marblehead.* Fourteen vessels on shore.—*Gloucester.* Vessels ashore, and buildings blown down.—*Danvers.* Storm violent, not greatly destructive. Pear tree imported and transplanted by Gov. Endicott in 1680 stripped of half its branches. Oaks that braved the tempest one hundred years thrown down.—*Andover.* Spray of salt water reached it, giving every thing it descended upon a saltish taste, and blighting every fibre of vegetation.—*Newburyport.* Ornamental trees suffered much; buildings injured.—*Ipswich.* Less damage done to vessels than other parts of our shore.—*Lynn.* Buildings suffered.—*Wenham.* Steeple blown down.—*Saugus.* Severe, trees blown down, barns, &c.—*Wells.* One man killed by falling of a tree.

55. I subjoin the table which I constructed, at the time of collecting the accounts, for the purpose of presenting a view of the storm simultaneously in different places, during the forenoon of the 23d September, 1815.

View of the Hurricane in the forenoon of Saturday, Sept. 23d, 1815.

TIME.	Amherst.	Salem.	Boston.	Worcester.	Providence.	NewBedford.	Fair Haven.	Newport.	Stonington.	Lyme.	New London.	Bridgeport.	New York.	Philadelphia.
A.M. 6 o'clock,		E.	E.	N. E.	N. E.	S. E.	S. E.		N. E.	N. E.	N. E.	N. E.	N.	N.W.
" 7 "		E.	E.	N. E.	N. E.	S. E.	S. E.		N. E.	N. E.	N. E.	N. E.	N.	N.W.
" 8 "		E.	E.	N. E.	S. E.	S. E.	S. E.		N. E.	S. E.	E.	N. E.	N.	N.W.
" 9 "		E.	E.	N. E.	S. E.	S. E.	S.	S.E. by S.	N. E.	S. E.	E.	N. E.	N.N.W.	
" 9½ "											S. E.			
" 10 "		E.	E.	N. E.	S. E.	S. E.	S.	S.E. by S.	S. E.		S. E.	N. E.	S. W.	
" 10¼ "				S. E.										
" 10½ "		E.	S. E.	S. E.	S. E.	S. E.	S.	S.E. by S.			S. E.	N. E.		
" 11 "		S. E.	S. E.		S. E.	S. E.	S.	S.E. by S.			S. E.	N.W.		
" 11½ "	S. E.	S. E.	S. E.		S. E.	S. E.	S.				S. E.			
M. 12 "		S. E.	S. E.		S. E.	S. E.	S. W.							
P.M. 1 "		S. E.	S. E.				S. W.							
" 1½ "		S. W.	S. W.				S. W.							
" 2 "							S. W.							

56. The following facts appear to me to be established by the foregoing accounts.

1st. The hurricane commenced in the West Indies, and moved northward at the rate of twelve or fifteen miles an hour.

2d. Its course from St. Barts was about W. N. W. to Turks Island, and thence to Boston (nearly on the same meridian) it was a curve convex to the west. (See account of *Schr. Sally*, lat. 37°, lon. 76°, for the most western point of the curve.)

3d. Previous to the arrival of the hurricane in New England, a N. E. storm had prevailed along the Atlantic coast for more than twenty four hours. (See accounts, New York, New London, Norwich, Worcester, and Boston.)

4th. For some hours previous to the hurricane, there was a great and rapid condensation of vapor, producing a heavy fall of rain in the line of the N. E. storm. (See accounts, Philadelphia, New York, New London, Norwich, Worcester, Boston and Troy.)

5th. The hurricane (that is, the violent blow) was mostly from the S. E., blowing into and at right angles to the N. E. storm, at its southern termination.

6th. As the S. E. wind approached the line of the N. E. storm, it was deflected into an E. wind. (See table, Salem, Boston, and New London.)*

7th. The general form of the hurricane in and about New England was that of an eccentric ellipse, with its longest diameter N. E. and S. W.; wind blowing N. E. on the N. W. side; N. N. W. and W. N. W. at its south end; S. E. on its S. E. side,† curving into an E. wind at its junction with the N. E. current; wind blowing from S. at the easternmost part of the hurricane. The whole body of the hurricane, in this form, moved to the north nearly on the meridian.

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* May we not rather suppose that the more violent S. E. storm pursued its own course of rotation in this direction by E. and N. E. without regard to the previous N. E. wind which it had superseded?—Eds.

† Did not this S. E. wind pertain more nearly to the centre of the path of the hurricane? For we find that the ship Prudence, twenty leagues from St. George's Shoals, had a tremendous swell from S. W., which would appear to have been produced on the S. E. margin of the gale.—Eds.

ART. VI.—*A New Method of determining the quantity of Nitrogen in Organic Compounds;* by Drs. VARRENTRAPP and WILL. Translated from the original in the Annalen der Chemie und Pharmacie, by J. LAWRENCE SMITH, M. D. of Charleston, S. C.

Messrs. Editors—In this letter I send you a translation of such parts of the original article of Drs. V. and W. as may enable one to thoroughly understand this valuable addition to what is already known upon the subject of organic analyses; neither the first nor the latter parts are here taken notice of, as the one is merely a detached account of the various processes that have been previously employed to estimate the quantity of nitrogen in organic compounds, and the other, the mention of some analyses made with the view of comparison.

"This method has for its basis the peculiar action of the hydrated fixed alkalies, upon organic substances containing nitrogen, when subjected to a high temperature. It consists in the separation of nitrogen in the form of ammonia, and estimating the latter, either under the form of the muriate of platinum and ammonia, or from metallic platinum."

"If one melts an organic substance free from nitrogen, together with the hydrate of potash, the water of the potash becomes decomposed, (as Gay Lussac has shown,) its oxygen combines with the carbon and hydrogen of the organic matter, and its hydrogen passes off in the form of gas."

For a perfect result of the above nature a high temperature is required, as well as a considerable excess of potash.

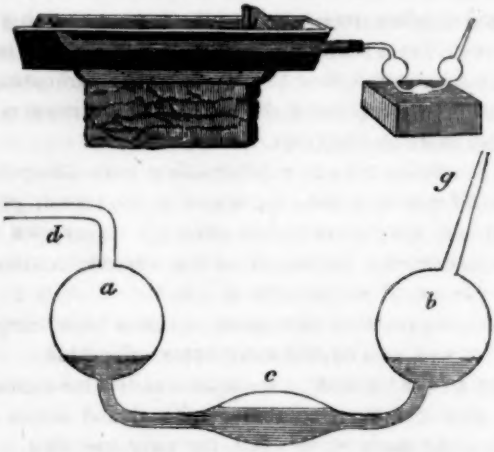
"When on the contrary, the organic substance contains nitrogen, the free hydrogen combines with it and forms ammonia. Since the observation of this fact, the only use that it has been put to, has been that of ascertaining the presence of nitrogen in an organic compound."

The first and chief difficulty that presented itself to Drs. V. and W. in making use of this method to estimate the quantity of nitrogen, was that when a substance was very rich in that element, the whole of it would not be converted into ammonia, but that a portion by combining with the carbon formed cyanogen, which would pass off or be converted into hydrocyanic acid, and the latter unite itself to the potash. But upon experi-

ment these gentlemen found, by employing a sufficient excess of hydrated alkali and a temperature not too low, that every cyanide and all other substances that contained nitrogen not under the form of nitric acid, would become decomposed by this means, and all the nitrogen be converted into ammonia.

"Our method, which is based upon the peculiar property already stated of all substances containing nitrogen, in which this element does not exist in combination, under the form of nitric acid, consists in the complete interception of the ammonia, by means of hydrochloric acid, and subsequently weighing it in a solid form as chloride of platinum and ammonia."

The apparatus used by Drs. V. and W. is such as is represented in the annexed figures. It consists of a furnace, such as is ordinarily used in organic analyses, with a tube of hard glass drawn out at its closed extremity. The length of this tube should be



from twelve to fifteen inches, and its inner diameter about three lines. To the open end of this tube is attached an apparatus, somewhat similar to Liebig's alkali bulbs, but differently constructed to facilitate the pouring out of the liquid, which is placed within it. It is composed of three bulbs *a*, *b*, *c*, the two first being about one and a quarter inches in diameter, and the latter about five lines. The tube connecting these bulbs is about a line in diameter, and drawn out at its extremity *g* by means of a spirit lamp. The bulbs are filled to about the height represented in the figure, with *pure*

hydrochloric acid of the ordinary strength, and this part of the operation is done by applying the mouth to the extremity *d*, and drawing the acid through the extremity *g*.

"As means of decomposing the organic substance containing nitrogen, that is to say, of oxydizing its carbon and hydrogen, we make use of the hydrate of potash, or soda mixed with caustic lime, in such proportions, that the mixture when exposed to a strong heat will not melt, but only slightly run together. This mixture has the advantage of being easily pulverized, and of not attracting moisture very rapidly, and generally it is managed with the same facility as the bioxide of copper or the chromate of lead. We have preferred latterly the mixture containing soda, in preference to that with potash; because the hydrate of soda, on account of its smaller atomic weight, contains more water, and consequently more oxydizing material than the same weight of the hydrate of potash. Also the mixture of the hydrate of soda and lime attracts moisture less readily than that of potash and lime; and moreover, the hydrate of soda requires but twice its weight of anhydrous lime to form a mixture that will not melt at a red heat. For one part of potash three of lime are necessary. The easiest method of preparing it, is to slake the lime in a solution of known concentration, of either of the above alkalies; heat the mixture to redness in a crucible, and rub it to a fine powder. Or we may rub up quickly one of the alkalies (first melted and then cooled) in a warm mortar, and then mix intimately with it anhydrous lime in the proper proportion. In this latter case, the lime must be first slaked and then heated to redness as a fine powder before being used. The mixture prepared in either of the above ways is once more heated to redness to drive away all mixture and then preserved in a well stopped phial."

In performing an analysis, the process is as follows:

"The burning tube, clean and dry, is half filled with the alkaline mixture, and that is the measured quantity to be mixed with the substance about to be analyzed, which is first dried and weighed in the ordinary method. The quantity of the substance employed should vary according as it contains more or less nitrogen. When rich in this element about three grains, but when its quantity is quite small about six grains should be used."

"The mixture of the weighed substance with the measured quantity of the alkaline powder, is done in a porcelain mortar

with a flat bottom, (which has been previously slightly warmed,) and with a very gentle movement of the pestle. If these precautions are strictly observed, no loss of material will result by its sticking to the sides of the mortar, or bottom of the pestle. If the mixture be pressed too hard, or if it be rubbed too fine, or if the mortar be not perfectly dry, a portion of the substance will adhere to its sides. After introducing the mixture into the burning tube in the ordinary way, the mortar is rinsed with the alkaline powder, with which we fill the tube to within an inch of its mouth, in which is placed a loose stopper of asbestus that has been heated to redness and cooled."

The use of the asbestus is to prevent the gas that is generated, from projecting any of the powdered alkali into the apparatus containing the hydrochloric acid, which accident would of course cause a serious error in the result.

"With a tight cork we unite the tube containing the mixture to the acid apparatus, and by warming slightly the bulb *a*, by bringing an ignited coal beneath it, we are able to ascertain whether the apparatus is tight, for if so, the liquid will be chased from the bulb *a*. The tube is now heated as in the case of ordinary organic analysis, by placing ignited coals at the front part of the furnace, that contains none of the organic substance. The cork must be kept as warm as possible, so that it may contain no moisture, which by absorbing ammonia, may cause a loss of nitrogen. So soon as the first part of the tube is red hot, we carry the fire slowly back upon that part containing the substance."

"Carbonic acid is formed, the oxygen in the water of the hydrated alkali combining with the carbon of the substance, the free hydrogen combines with its nitrogen, and ammonia is formed, which is absorbed by the hydrochloric acid. Hydrogen or carburetted hydrogen (according as the substance contains more or less carbon) is evolved, and this passing through the acid without being absorbed, enables us to see the progress of the burning."

"It may be well to remark, that a continued current of gas is evolved, but there need be no apprehension of the escape of the ammonia, for its absorption is so complete and goes on with such rapidity, that one has rather to apprehend a recoil of the fluid; if the current of gas is only stopped for a moment, the fluid rises in the bulb *a*, and if the fire be carelessly attended to, it will enter the tube *d*, and thence the burning tube, rendering the experiment useless."

Drs. V. and W. state that when the body contains a large portion of nitrogen, this accident is more to be apprehended, and they recommend the mixing of an equal weight of some substance free from nitrogen, as for instance, sugar, with the body containing nitrogen, and with the alkali lime, which by forming gases not absorbable by the acid, will cause no error, and prevent any accident.

"After the whole length of the tube is red hot and gas no longer generated, when all the carbon of the substance is burnt, and the mixture in the tube appears white, the small end of the burning tube is broken, and a quantity of air drawn through the burning tube, and absorption apparatus, in order to bring all the ammonia in the tube in contact with the hydrochloric acid; the air is drawn through by means of the extremity *g*, a small tube containing potash being placed between it and the mouth, to prevent one from inhaling the acid vapors."

"We proceed to analyze fluids exactly as in the burning with the binoxide of copper. A little of the alkali lime is first introduced into the burning tube, and upon that is dropped the little bulb, containing the known quantity of the fluid to be analyzed, with its capillary extremity broken off, and then fill the tube as before with the alkali lime. The operation goes on more regularly, if we commence by heating the first third of the tube to redness, and then warming that part of the tube containing the bulb, from which the fluid is expelled, which disseminates itself over the middle part of the tube, without being decomposed; if we now carry the fire from before backwards, a gentle and constant current of gas will be evolved."

"After the burning is complete, and the air passed through the apparatus, the contents of the absorption apparatus are emptied into a small porcelain capsule. With a dropping tube we introduce first into the apparatus, a mixture of alcohol and ether, which must be shaken backwards and forwards, and then added to the first fluid. The bulbs are now washed with water, until it affords no acid reaction, and all the washings added to the hydrochloric acid containing the hydrochlorate of ammonia. The washing with alcohol and ether has for its object the dissolving of any carburetted hydrogen which may be formed during the burning, and by collecting in the bulbs would prevent the water from coming perfectly in contact with the sides, and thereby ren-

dering a complete washing difficult. Seldom is more than an ounce to an ounce and a half of fluid needed for the removal of all the hydrochlorate of ammonia."

"To this fluid, now containing hydrochlorate of ammonia, an excess of a pure solution of the chloride of platinum is added, and it is then evaporated to dryness, either first by the aid of a spirit and then by a water bath, or entirely by the latter means. From a well conducted burning, the chloride of platinum and ammonia that is obtained, is always beautifully yellow; but if the material contained a great deal of carbon, or was burnt with difficulty, then the chloride of platinum and ammonia is of a darker color. This color though, has little or no effect upon the accuracy of the result, supposing the washing to have been carefully done."

"Upon the residue in the porcelain capsule, after it has cooled, a mixture of two parts of absolute alcohol and one of ether is poured. This dissolves the excess of the chloride of platinum, and no part of the chloride of platinum and ammonia. It is immediately known, whether there has been an excess of the chloride of platinum, by the fluid assuming a yellow color; if it is colorless, then there has been a deficiency."

"The washing of the residue is performed easiest by holding the capsule, after the fluid has been poured upon a weighed filter, perpendicularly over the same, and washing the precipitate in, with the solution of alcohol and ether. The filter must be washed by the same mixture until it passes without color or acid reaction. The precipitate and filter are dried on a covered capsule or weighed tube at 212° Fah. and weighed. It is well, in order to continue the result, to decompose the chloride of platinum and ammonia by heat, and out of the residue obtained, calculate again the quantity of nitrogen, and if the chloride of platinum was pure, the quantity of nitrogen calculated by this latter method will not differ from the double chloride."

In decomposing the chloride of platinum and ammonia, it must be done by heating it, enveloped in its filter, gradually to redness; for if it be done too hastily, the vapor of the muriate of ammonia and chlorine will remove mechanically some of the platinum. The chloride of platinum used in this process, must be perfectly pure, and Drs. V. and W. state, that it is difficult to obtain spongy platinum without a trace of muriate of ammonia, by simply de-

composing the chloride of platinum and ammonia by heat. They therefore recommend that the spongy platinum used to form the chloride, should be well washed with hot distilled water, before being dissolved in the nitro-hydrochloric acid.

"If we weigh the nitrogen as the chloride of platinum and ammonia, we have one hundred and seventy seven parts of it for every two thousand seven hundred and eighty eight of the double chloride, but if as metallic platinum, one hundred and seventy seven parts for every twelve hundred and thirty three of the metal."

"The weight of the chloride of platinum and ammonia remains constant under a long continued drying at 212° Fah., as well as that of the filter, after it has been completely washed. If there be a trace of acid remaining on the filter it becomes dark and friable."

I believe that now all the important steps in this operation have been fully detailed, and I shall conclude with a translation of a letter of Prof. Liebig's, that was attached to the original publication, in order that it may be seen, how highly this new method of estimating the quantity of nitrogen in organic bodies is esteemed by that distinguished chemist.

"I have had the pleasure of being constantly present at the development of the experiments by which Drs. V. and W. by degrees have arrived at the method, which they have here described. I regard this new method of estimating nitrogen as one of the most important improvements in organic analysis, because it determines the quantity of nitrogen with a certainty and precision which until now were wanting. The whole opreation is completed in a few hours, and with all the accuracy of the determination of the carbonic acid. I doubt not that this apparatus will very soon replace the ordinary method, to the contentment of all analytical chemists.—J. L."

The little experience and observation I have had of it, verify the opinion of Prof. Liebig, and in simplicity it is every thing that can be desired.

Geissen, November, 1841.

ART. VII.—*A Letter to William Whewell, Professor of Moral Philosophy in the University of Cambridge, England, in reply to certain allegations and arguments advanced in a pamphlet entitled a Demonstration that all Matter is Heavy;** by ROBERT HARE, M. D., Professor of Chemistry in the University of Pennsylvania.

1. *Dear Sir*—I thank you for your kind attention in sending me a copy of your pamphlet entitled a "*Demonstration that all Matter is Heavy*," comprising a communication made to the Cambridge Philosophical Society.

2. I conceive that to demonstrate that all matter is heavy, is, in other words, to prove that all matter is endowed with attraction of gravitation, or that general property which, when it causes bodies to tend towards the centre of the earth, is called weight. Hence to assert that all matter is heavy, is no more than to say, that attraction of gravitation exists between all or any masses of matter.†

3. You say, "it may be urged that we have no difficulty in conceiving of matter which is not heavy." I have no hesitation in asserting, that there should be no difficulty in entertaining such a conception; since I cannot understand why any two masses may not be as readily conceived to *repel* as to *attract* each other, or *neither to attract nor to repel.* Is it not easier to imagine two remote masses indifferent to each other, than that they act upon each other? Is any thing more difficult to understand than that a body can act where it is not?

4. It is also mentioned by you, that it may be urged "*that inertia and weight are two separate properties of matter.*" Now I will not only urge, but also, with all due deference, will undertake to show, that the existence of inertia may as well be proven, and its quantity estimated, by means of repulsion as by means of attraction.

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* Transactions of the Cambridge Philosophical Society.

† We have thought that Dr. Hare's letter would be better understood by our readers, if we republished the "Demonstration" of Prof. Whewell, as it has probably been seen by few persons in America. It will accordingly be found in full at the end of Dr. Hare's letter.—EDS.

5. Suppose two bodies, A and B, to be endowed with reciprocal attraction; or, in other words, to gravitate towards each other. Being placed at a distance, and then allowed to approach, if, after any given time, it were found that they had moved severally any ascertained distances, evidently their relative inertias would be considered as inversely as those distances.

6. In the next place, let us suppose two bodies, X and Y, endowed with the opposite force of reciprocal repulsion, to be placed in proximity, and then allowed to fly apart. The distances run through by them severally, being, at any given time, determined, might not their respective inertias be taken to be inversely as those distances; so that the question would be as well ascertained in this case, as in that above stated in which gravitation should be resorted to as the test?

7. It seems to me that this question is sufficiently answered, in the affirmative, in your second paragraph, page 7, (p. 269,) in which you allege, that "*one body has twice as much inertia as another, if when the same force acts upon it for the same time, it acquires but half the velocity. This is the fundamental conception of inertia.*"

8. In the third paragraph, fourth page, (p. 261,) you say, "*that the quantity of matter is measured by those sensible properties of matter which undergo quantitative addition, subtraction and division, as the matter is added, subtracted or divided, the quantity of matter cannot be known in any other way; but this mode of measuring the quantity of matter in order to be true at all, must be true universally.*"

9. Also your fourth paragraph, fifth page, (p. 268,) concludes with this allegation, "*and thus we have proved, that if there be any kind of matter which is not heavy, the weight can no longer avail us, in any case to any extent, as the measure of the quantity of matter.*"

10. In reply to these allegations let me inquire, cannot a matter exist of which the sensible properties do not admit of being measured by human means? Because some kinds of matter can be measured by "those sensible qualities which undergo quantitative addition, subtraction and division," does it follow that there may not be matter which is incapable of being thus measured? And wherefore would the method of obtaining phi-

losophical truth be "futile" in the one case, because inapplicable in the other? Because the inertias of A and B have been discovered, by means of their gravitation, does it follow that the inertias of X and Y cannot be discovered by their self-repellent power? Why should the inapplicability of gravitation in the one case render its employment futile in the other?

11. It is self-evident, that matter without weight cannot be estimated by weighing, but I deny that on that account such weightless matter may not be otherwise estimated. The inertias of A and B cannot be better measured by gravitation than those of X and Y by repulsion, as already shown.

12. You seem to infer, in paragraph second, page sixth, (p. 268,) that we should be equally destitute of the means of measuring matter accurately, "*were any kind of matter heavy indeed, but not so heavy, in proportion to its quantity of matter, as other kinds.*"

13. If in the case of all matter weight be admitted to be the only measure of quantity, it were inconsistent to suppose any given quantity of matter, of any one kind, to have less weight than an equal quantity of another kind; but upon what other than a conventional basis is it to be assumed, that there is more matter in a cubic inch of platinum than in a cubic inch of tin; in a cubic inch of mercury than in a cubic inch of iron? Judging by the chemical efficacy of the masses, although the weight of mercury is to that of iron as 13.6 is to 8, there are more equivalents of the latter than the former in any given bulk, since by weight twenty-eight parts of iron are equivalent to two hundred and two parts of mercury.

14. Weight is one of the properties of certain kinds of matter, and has been advantageously resorted to, in preference to any other property, in estimating the quantity of the matter to which it appertains. Nevertheless, measurement by bulk is found expedient or necessary in many cases. But may we not appeal to any general property which admits of being measured or estimated? Faraday has inferred that the quantity of electricity, is as the quantity of gas which it evolves. Light has been considered as proportional in quantity to the surface which it illuminates with a given intensity at a certain distance. The quantity of caloric has been held to be directly as the weight of water which it will render aëriform; and has also been estima-

ted by the degree of its expansive or thermometric influence. What scale-beam is more delicate than the thermoscope of Melloni?

15. In the last paragraph but one, seventh page, (p. 270,) you suggest, that "*perhaps some persons might conceive that the identity of weight and inertia is obvious at once, for both are merely resistance to motion; inertia, resistance to all motion, or change of motion; weight, resistance to motion upwards.*"

16. I am surprised that you should think the opinion of any person worthy of attention, who should entertain so narrow a view of weight, as antagonist of momentum, as that above quoted, "*that it is a resistance to motion upwards.*" Agreeably to the definition, given at the commencement of the letter, weight, in its usual practical sense, is only one case of the general force which causes all ponderable masses of matter to gravitate towards each other, and which is of course liable to resist any conflicting motion, whatever may be the direction. When in the form of solar attraction, it overcomes that inertia of the planets which would otherwise cause them to leave their orbits, does gravitation "*resist motion upwards?*"

17. In the next paragraph you allege, that "*there is a difference in these two kinds of resistance to motion. Inertia is instantaneous, weight is continuous resistance.*"

18. It is to this allegation I object, that as you have defined inertia to be "*resistance to motion, or to change of motion,*" it follows that it can be instantaneous only where the impulse which it resists is instantaneous. It cannot be less continuous than the force by which it is overcome.

19. Gravity has been considered as acting upon falling bodies by an infinity of impulses, each producing an adequate acceleration; but to every such accelerating impulse, producing of course a "*change of motion,*" will there not be a commensurate resistance from inertia? and the impulses and resistances being both infinite, will not one be as continuous as the other?

20. I have already adverted to inertia as the continuous antagonist of solar attraction in the case of revolving planets.

21. Agreeably to Mossotti, the creation consists of two kinds of matter, of which the homogeneous particles are mutually repellent, the heterogeneous mutually attractive. Consistently with

this hypothesis, *per se*, any matter must be imponderable; being endowed with a property the very opposite of attraction of gravitation. This last mentioned property exists between masses consisting of both kinds of particles, so far as the attraction between the heterogeneous atoms predominates over the repulsion between those which are homogeneous. It would follow from these premises, that all matter is ponderable or otherwise, accordingly as it may be situated.

22. Can the ether by which, according to the undulatory theory, light is transmitted, consist of ponderable matter? Were it so, would it not be attracted about the planets with forces proportioned to their weight, respectively? and becoming of unequal density, would not the diversity in its density, thus arising, affect its undulations, as the transmission of sound is influenced by any variations in the density of the aëriform fluid by which it is propagated?

With esteem,

I am yours truly,

ROBERT HARE.

Demonstration that all Matter is Heavy. By the Rev. WILLIAM WHEWELL, B. D., *Fellow of Trinity College and Professor of Moral Philosophy.* [Read February 22d, 1841.]

"The discussion of the nature of the grounds and proofs of the most general propositions which the physical sciences include, belongs rather to metaphysics than to that course of experimental and mathematical investigation by which the sciences are formed. But such discussions seem by no means unfitted to occupy the attention of the cultivators of physical science. The ideal, as well as the experimental side of our knowledge, must be carefully studied and scrutinized, in order that its true import may be seen; and this province of human speculation has been perhaps of late unjustly depreciated and neglected by men of science. Yet it can be prosecuted in the most advantageous manner by them only: for no one can speculate securely and rightly respecting the nature and proofs of the truths of science without a steady possession of some large and solid portions of such truths. A man must be a mathematician, a mechanical philosopher, a natural historian, in order that he may philosophize well concerning mathematics, and mechanics,

and natural history; and the mere metaphysician who without such preparation and fitness sets himself to determine the grounds of mathematical or mechanical truths, or the principles of classification, will be liable to be led into error at every step. He must speculate by means of general terms, which he will not be able to use as instruments of discovering and conveying philosophical truth, because he cannot, in his own mind, habitually and familiarly, embody their import in special examples.

"Acting upon such views, I have already laid before the Philosophical Society of Cambridge essays on such subjects as I here refer to; especially a memoir "On the Nature of the Truth of the Laws of Motion," which was printed by the Society in its Transactions. This memoir appears to have excited in other places, notice of such a kind as to shew that the minds of many speculative persons are ready for and inclined towards the discussion of such questions. I am therefore the more willing to bring under consideration another subject of a kind closely related to the one just mentioned.

"The general questions which all such discussions suggest, are (in the existing phase of English philosophy) whether certain proposed scientific truths, (as the laws of motion,) be *necessary* truths; and if they are necessary, (which I have attempted to shew that in a certain sense they are,) *on what ground* their necessity rests. These questions may be discussed in a general form, as I have elsewhere attempted to shew. But it may be instructive also to follow the general arguments into the form which they assume in special cases; and to exhibit, in a distinct shape, the incongruities into which the opposite false doctrine leads us, when applied to particular examples. This accordingly is what I propose to do in the present memoir, with regard to the proposition stated at the head of this paper, namely, that *all matter is heavy.*

"At first sight it may appear a doctrine altogether untenable to assert that this proposition is a necessary truth: for it may be urged, we have no difficulty in conceiving matter which is not heavy; so that matter without weight is a conception not inconsistent with itself; which it must be if the reverse were a necessary truth. It may be added, that the possibility of conceiving matter without weight was shewn in the controversy which ended in the downfall of the phlogiston theory of chemical composition; for some of the reasoners on this subject asserted phlogiston to be a body with positive levity instead of gravity, which hypothesis, however false, shews that such a supposition is possible. Again, it may be said that *weight* and *inertia* are two separate properties of matter; that mathematicians measure the quantity of matter by the inertia, and that we learn by experiment only that the

weight is proportional to the inertia; Newton's experiments with pendulums of different materials having been made with this very object.

"I proceed to reply to these arguments. And first, as to the possibility of conceiving matter without weight, and the argument thence deduced, that the universal gravity of matter is not a necessary truth, I remark, that it is indeed just to say that we cannot even distinctly conceive the contrary of a necessary truth to be true; but that this impossibility can be asserted only of those perfectly distinct conceptions which result from a complete developement of the fundamental idea and its consequences. Till we reach this stage of developement, the obscurity and indistinctness may prevent our perceiving absolute contradictions, though they exist. We have abundant store of examples of this even in geometry and arithmetic; where the truths are universally allowed to be necessary, and where the relations which are impossible, are also inconceivable, that is, not conceivable distinctly. Such relations, though not distinctly conceivable, still often appear conceivable and possible, owing to the indistinctness of our ideas. Who, at the first outset of his geometrical studies, sees any impossibility in supposing the side and the diagonal of a square to have a common measure? Yet they can be rigorously proved to be incommensurable, and therefore the attempt distinctly to conceive a common measure of them must fail. The attempts at the geometrical duplication of the cube, and the supposed solutions, (as that of Hobbes) have involved absolute contradictions; yet this has not prevented their being long and obstinately entertained by men, even of minds acute and clear in other respects. And the same might be shewn to be the case in arithmetic. It is plain, therefore, that we cannot, from the supposed possibility of conceiving matter without weight, infer that the contrary may not be a necessary truth.

"Our power of judging, from the compatibility or incompatibility of our conceptions, whether certain propositions respecting the relations of ideas are true or not, must depend entirely, as I have said, upon the degree of developement which such ideas have undergone in our minds. Some of the relations of our conceptions on any subject are evident upon the first steady contemplation of the fundamental idea by a sound mind: these are the *axioms* of the subject. Other propositions may be deduced from the axioms by strict logical reasoning. These propositions are no less *necessary* than the axioms, though to common minds their *evidence* is very different. Yet as we become familiar with the steps by which these ulterior truths are deduced from the axioms, *their* truth also becomes evident, and the contrary becomes inconceivable. When a person has familiarized himself with the first twenty-six

propositions of Euclid, and not till then, it becomes evident to him, that parallelograms on the same base and between the same parallels are equal; and he cannot even conceive the contrary. When he has a little further cultivated his geometrical powers, the equality of the square on the hypothenuse of a right-angled triangle to the squares on the sides, becomes also evident; the steps by which it is demonstrated being so familiar to the mind as to be apprehended without a conscious act. And thus, the contrary of a necessary truth cannot be distinctly conceived; but the incapacity of forming such a conception is a condition which depends upon cultivation, being intimately connected with the power of rapidly and clearly perceiving the connection of the necessary truth under consideration with the elementary principles on which it depends. And thus, again, it may be that there is an absolute impossibility of conceiving matter without weight; but then, this impossibility may not be apparent, till we have traced our fundamental conceptions of matter into some of their consequences.

"The question then occurs, whether we can, by any steps of reasoning, point out an inconsistency in the conception of matter without weight. This I conceive we may do, and this I shall attempt to shew.

"The general mode of stating the argument is this:—the quantity of matter is measured by those sensible properties of matter which undergo quantitative addition, subtraction and division, as the matter is added, subtracted and divided. The quantity of matter cannot be known in any other way. But this mode of measuring the quantity of matter, in order to be true at all, must be universally true. If it were only partially true, the limits within which it is to be applied would be arbitrary; and therefore the whole procedure would be arbitrary, and, as a method of obtaining philosophical truth, altogether futile.

"We may unfold this argument further. Let the contrary be supposed, of that which we assert to be true: namely, let it be supposed that while all other kinds of matter are heavy, (and of course heavy in proportion to the quantity of matter,) there is one kind of matter which is absolutely destitute of weight; as, for instance, phlogiston, or any other element. Then where this *weightless* element (as we may term it) is mixed with *weighty* elements, we shall have a compound, in which the weight is no longer proportional to the quantity of matter. If, for example, 2 measures of heavy matter unite with 1 measure of phlogiston, the weight is as 2, and the quantity of matter as 3. In all such cases, therefore, the weight ceases to be the measure of the quantity of matter. And as the proportion of the weighty and the weightless matter may vary in innumerable degrees in such compounds, the weight affords no criterion at all of the quantity of matter in them.

And the smallest admixture of the weightless element is sufficient to prevent the weight from being taken as the measure of the quantity of matter.

"But on this hypothesis, how are we to distinguish such compounds from bodies consisting purely of heavy matter? How are we to satisfy ourselves that there is not, in every body, some admixture, small or great, of the weightless element? If we call this element *phlogiston*, how shall we know that the bodies with which we have to do are, any of them, absolutely free from phlogiston?

"We cannot refer to the weight for any such assurance; for by supposition the presence and absence of phlogiston makes no difference in the weight. Nor can any other properties secure us at least from a very small admixture; for to assert that a mixture of 1 in 100 or 1 in 10 of phlogiston would always manifest itself in the properties of the body, must be an arbitrary procedure, till we have proved this assertion by experiment; and we cannot do this till we have learnt some mode of measuring the quantities of matter in bodies and parts of bodies; which is exactly what we question the possibility of, in the present hypothesis.

"Thus, if we assume the existence of an element, *phlogiston*, devoid of weight, we cannot be sure that every body does not contain some portion of this element; while we see that if there be an admixture of such an element, the weight is no longer any criterion of the quantity of matter. And thus we have proved, that if there be any kind of matter which is not heavy, the weight can no longer avail us, *in any case or to any extent*, as a measure of the quantity of matter.

"I may remark, that the same conclusion is easily extended to the case in which phlogiston is supposed to have absolute levity; for in that case, a certain mixture of phlogiston and of heavy matter would have no weight, and might be substituted for phlogiston in the preceding reasoning.

"I may remark also, that the same conclusion would follow by the same reasoning, if any kind of matter, instead of being void of weight, were heavy indeed, but not *so* heavy, in proportion to its quantity of matter, as other kinds.

"On all these hypotheses there would be no possibility of measuring quantity of matter by weight at all, in any case, or to any extent.

"But it may be urged, that we have not yet reduced the hypothesis of matter without weight to a contradiction; for that mathematicians measure quantity of matter, not by weight, but by the other property, of which we have spoken, inertia.

"To this I reply, that, practically speaking, quantity of matter is always measured by weight, both by mechanicians and chemists: and as we have proved that this procedure is utterly insecure in all cases, on the hypothesis of weightless matter, the practice rests upon a conviction that the hypothesis is false. And yet the practice is universal. Every experimenter measures quantity of matter by the balance. No one has ever thought of measuring quantity of matter by its inertia practically; no one has constructed a measure of quantity of matter in which the matter produces its indications of quantity by its motion. When we have to take into account the inertia of a body, we inquire what its weight is, and assume this as the measure of the inertia; but we never take the contrary course, and ascertain the inertia first in order to determine by that means the weight.

"But it may be asked, Is it not then true, and an important scientific truth, that the *quantity of matter* is measured by the *inertia*? Is it not true, and proved by experiment, that the *weight* is *proportional* to the *inertia*? If this be not the result of Newton's experiments mentioned above, what, it may be demanded, do they prove?

"To these questions I reply: It is true that quantity of matter is measured by the inertia, for it is true that inertia is as the quantity of matter. This truth is indeed one of the laws of motion. That weight is proportional to inertia is proved by experiment, as far as the laws of motion are so proved: and Newton's experiments prove one of the laws of motion, so far as any experiments can prove them, or are needed to prove them.

"That inertia is proportional to weight, is a law equivalent to that law which asserts, that when pressure produces motion in a given body, the velocity produced in a given time is as the pressure. For if the velocity be as the pressure, when the body is given, the velocity will be constant if the inertia also be as the pressure. For the inertia is understood to be that property of bodies to which, *ceteris paribus*, the velocity impressed is *inversely* proportional. One body has twice as much inertia as another, if, when the same force acts upon it for the same time, it acquires but half the velocity. This is the fundamental conception of *inertia*.

"In Newton's pendulum experiments, the pressure producing motion was a certain resolved part of the weight, and was proportional to the weight. It appeared by the experiments, that whatever were the material of which the pendulum was formed, the rate of oscillation was the same; that is, the velocity acquired was the same. Hence the inertia of the different bodies must have been in each case as the weight: and thus this assertion is true of all different kinds of bodies.

"Thus it appears that the assertion, that inertia is universally proportional to weight, is equivalent to the law of motion, that the velocity is as the pressure. The conception of inertia (of which, as we have said, the fundamental conception is, that the velocity impressed is inversely proportional to the inertia,) connects the two propositions so as to make them identical.

"Hence our argument with regard to the universal gravity of matter brings us to the above law of motion, and is proved by Newton's experiments in the same sense in which that law of motion is so proved.

"Perhaps some persons might conceive that the identity of weight and inertia is obvious at once; for both are merely resistance to motion;—inertia, resistance to all motion (or change of motion)—weight, resistance to motion upwards.

"But there is a difference in these two kinds of resistance to motion. Inertia is instantaneous, weight is continuous resistance. Any momentary impulse which acts upon a free body overcomes its inertia, for it changes its motion: and this change once effected, the inertia opposes any return to the former condition, as well as any additional change. The inertia is thus overcome by a momentary force. But the weight can only be overcome by a continuous force like itself. If an impulse act in opposition to the weight, it may for a moment neutralize or overcome the weight; but if it be not continued, the weight resumes its effect, and restores the condition which existed before the impulse acted.

"But weight not only produces rest, when it is resisted, but motion, when it is not resisted. Weight is measured by the reaction which would balance it; but when unbalanced, it produces motion, and the velocity of this motion increases constantly. Now what determines the velocity thus produced in a given time, or its rate of increase? What determines it to have one magnitude rather than another? To this we must evidently reply, *the inertia*. When weight produces motion, the inertia is the reaction which makes the motion determinate. The accumulated motion produced by the action of unbalanced weight is as determinate a condition as the equilibrium produced by balanced weight. In both cases the condition of the body acted on is determined by the opposition of the action and reaction.

"Hence inertia is the reaction which opposes the weight, when unbalanced. But by the conception of action and reaction, (as mutually determining and determined,) they are measured by each other: and hence the inertia is necessarily proportional to the weight.

"But when we have reached this conclusion, the original objection may be again urged against it. It may be said, that there must be some

fallacy in this reasoning, for it proves a state of things to be necessary when we can so easily conceive a contrary state of things. Is it denied, the opponent may ask, that we can readily imagine a state of things in which bodies have no weight? Is not the uniform tendency of all bodies in the same direction not only not necessary, but not even true? For they do in reality tend, not with equal forces in parallel lines, but to a centre with unequal forces, according to their position: and we can conceive these differences of intensity and direction in the force to be greater than they really are; and can with equal ease suppose the force to disappear altogether.

"To this I reply, that certainly we may conceive the weight of bodies to vary in intensity and direction, and by an additional effort of imagination may conceive the weight to vanish: but that in all these suppositions, even in the extreme one, we must suppose the rule to be universal. If *any* bodies have weight, *all* bodies must have weight. If the direction of weight be different in different points, this direction must still vary according to the *law of continuity;* and the same is true of the intensity of the weight. For if this were not so, the rest and motion, the velocity and direction, the permanence and change of bodies, as to their mechanical condition, would be arbitrary and incoherent: they would not be subject to mechanical ideas; that is, not to ideas at all; and hence these conditions of objects would in fact be inconceivable. In order that the universe may be possible, that is, may fall under the conditions of intelligible conceptions, we must be able to conceive a body at rest. But the rest of bodies (except in the absolute negation of all force) implies the equilibrium of opposite forces. And one of these opposite forces must be a *general* force, as weight, in order that the universe may be governed by general conditions. And this general force, by the conception of force, may produce motion, as well as equilibrium; and this motion again must be determined, and determined by general conditions; which cannot be, except the communication of motion be regulated by an inertia proportional to the weight.

"But it will be asked, Is it then pretended that Newton's experiment, by which it was intended to prove inertia proportional to weight, does really prove nothing but what may be demonstrated *à priori?* Could we know, without experiment, that all bodies,—gold, iron, wood, cork,—have inertia proportional to their weight? And to this we reply, that experiment holds the same place in the establishment of this, as of the other fundamental doctrines of mechanics. Intercourse with the external world is requisite for developing our ideas; measurement of phenomena is needed to fix our conceptions and to render them precise; but the result of our experimental studies is, that we reach a position in which

our convictions do not rest upon experiment. We learn by observation truths of which we afterwards see the necessity. This is the case with the laws of motion, as I have repeatedly endeavored to shew. The same will appear to be the case with the proposition, that bodies of different kinds have their inertia proportional to their weight.

"For bodies *of the same kind* have their inertia proportional to their weight, both quantities being proportional to the quantity of matter. And if we compress the same quantity of matter into half the space, neither the weight nor the inertia is altered, because these depend on the quantity of matter alone. But in this way we obtain a body of *twice the density;* and in the same manner we obtain a body of any other density. Therefore whatever be the density, the inertia is proportional to the quantity of matter. But the mechanical relations of bodies cannot depend upon any difference of *kind*, *except* a difference of density. For if we suppose any fundamental difference of mechanical nature in the particles or component elements of bodies, we are led to the same conclusion, of arbitrary, and therefore, impossible, results, which we deduced from this supposition with regard to weight. Therefore all bodies of different density, and hence, all bodies whatever, must have their inertia proportional to their weight.

"Hence we see, that the propositions, that all bodies are heavy, and that inertia is proportional to weight, necessarily follow from those fundamental ideas which we unavoidably employ in all attempts to reason concerning the mechanical relations of bodies. This conclusion may perhaps appear the more startling to many, because they have been accustomed to expect that fundamental ideas and their relations should be self-evident at our first contemplation of them. This, however, is far from being the case, as I have already shewn. It is not the *first*, but the most complete and developed condition of our conceptions which enables us to see what are axiomatic truths in each province of human speculation. Our fundamental ideas are necessary conditions of knowledge, universal forms of intuition, inherent types of mental development; they may even be termed, if any one chooses, results of connate intellectual tendencies; but we cannot term them *innate* ideas, without calling up a large array of false opinions. For innate ideas were considered as capable of composition, but by no means of simplification; as most perfect in their original condition; as to be found, if any where, in the most uneducated and most uncultivated minds; as the same in all ages, nations, and stages of intellectual culture; as capable of being referred to at once, and made the basis of our reasonings, without any special acuteness or effort: in all which circumstances the fundamental ideas of which we have spoken, are opposed to innate ideas so understood.

"I shall not, however, here prosecute this subject. I will only remark, that fundamental ideas, as we view them, are not only not innate, in any usual or useful sense, but they are not necessarily *ultimate* elements of our knowledge. They are the results of our analysis so far as we have yet prosecuted it; but they may themselves subsequently be analyzed. It may hereafter appear, that what we have treated as different fundamental ideas have, in fact, a connexion, at some point below the structure which we erect upon them. For instance, we treat of the mechanical ideas of force, matter, and the like, as distinct from the idea of substance. Yet the principle of measuring the quantity of matter by its weight, which we have deduced from mechanical ideas, is applied to determine the substances which enter into the composition of bodies. The idea of substance supplies the axiom, that the whole quantity of matter of a compound body is equal to the sum of the quantities of matter of its elements. The mechanical ideas of force and matter lead us to infer that the quantity both of the whole and its parts must be measured by their weights. *Substance* may, for some purposes, be described as that to which properties belong; *matter* in like manner may be described as that which resists force. The former involves the idea of permanent being; the latter the idea of causation. There may be some elevated point of view from which these ideas may be seen to run together. But even if this be so, it will by no means affect the validity of reasonings founded upon these notions, when duly determined and developed. If we once adopt a view of the nature of knowledge which makes necessary truth possible at all, we need be little embarrassed by finding how closely connected different necessary truths are; and how often, in exploring towards their roots, different branches appear to spring from the same stem. W. Whewell."

Grange, August 31, 1840.

Art. VIII.—*Integration of a particular kind of Differential Equations of the second order;* by Prof. Theodore Strong.

The equations which we propose to integrate, are $\frac{d^2y}{du^2}+\frac{2pq-q+1}{u}\frac{dy}{du}+q^2a^2b^2u^{2q-2}y=0$, (1), and $\frac{d^2y}{du^2}+\frac{2pq-q+1}{u}\frac{dy}{du}-q^2a^2b^2u^{2q-2}y=0$, (2), in which y and u are the only variable quantities, u being considered as the independent variable, whose differential (denoted by du) is supposed to be constant or invariable, and y is supposed to be a function of u.

(1) is taken from Vol. III, p. 537, second edition, of the Traité du Calcul Differentiel et du Calcul Integral, par S. F. Lacroix; and (2) is deduced from (1) by changing the sign of the last term, or which comes to the same thing, by changing b^2 in (1) into $-b^2$, that is, using $b\sqrt{-1}$ for b. We shall put $2pq-q+1=c$, (a), and shall use the characteristic, f_n^m, when prefixed to any differential expression, to signify that its integral is to be taken with reference to the variable, from its value n, to the value m; or n and m are two values of the variable, and the integral is supposed to be taken from the first limit n, to the second m.

Lacroix remarks that $y=\int_0^a dx(a^2-x^2)^{p-1}\cos.\, bu^q x$, is a particular value of y, which satisfies (1); in which u^q is to be regarded as constant in taking the integral, x and its functions being the only variables, and the integral is supposed to be taken from $x=0$ to $x=a$, which are the first and second limits of x.

We have also found by integrating (1) (by the method of sesies) that it is satisfied by the particular value of y, which is denoted by $y=u^{1-c}\int_0^a dv(a^2-v^2)^{-p}\cos.bu^q v$, when p is positive, and such that $1-p$ is positive, and not an indefinitely small quantity, and it is to be noted that v and its functions are considered as the only variables in taking the integral, u^q being regarded as constant. Hence if we use A and B to denote two arbitrary constants, we shall have (by the well known theory of integrals) for the complete value of y, the expression $y=\mathrm{A}\int_0^a dx(a^2-x^2)^{p-1}\cos.bu^q x+\mathrm{B}u^{1-c}\int_0^a dv(a^2-v^2)^{-p}\cos.bu^q v$, (b), in which p must not be considered as an indefinitely small quantity, and $1-p$ is positive and finite; since the limits of x and v are the same in (b), we may change v into x, and then the value of y may be put under the form $y=\int_0^a dx(a^2-x^2)^{p-1}\cos.bu^q x\left(\mathrm{A}+\mathrm{B}u^{1-c}(a^2-x^2)^{1-2p}\right)$, (c). If we put $1-c=e$, $1-2p=f$, or $c=1-e$, $2p=1-f$, (d), we get $u^{1-c}(a^2-x^2)^{1-2p}=u^e(a^2-x^2)^f$, and if we substitute the values of c and $2p$ from (d) in (a), we get by a slight reduction $f=\frac{e}{q}$, $\therefore u^e(a^2-x^2)^f=\left(u(a^2-x^2)^{\frac{1}{q}}\right)^e$, hence, when e is very small, using hyperbolic logarithms and rejecting

the terms which involve e^2, e^3, &c. we get (by the exponential theorem,) $u^{1-c}(a^2-x^2)^{1-2p}=1+e\log.u(a^2-x^2)^{\frac{1}{q}}$, which when substituted in (c) gives $y=\int_0^u dx(a^2-x^2)^{p-1}\cos.bu^q x\left(\mathrm{A}+\mathrm{B}+\mathrm{B}e\log.u(a^2-x^2)^{\frac{1}{q}}\right)$, in which (although e is supposed to be indefinitely small,) we may suppose that $\mathrm{A}+\mathrm{B}$ is yet represented by A', and $e\mathrm{B}$ by B', A', B' being arbitrary finite quantities; $\therefore y=\int_0^u dx(a^2-x^2)^{p-1}\cos.bu^q x\left(\mathrm{A}'+\mathrm{B}'\log.u(a^2-x^2)^{\frac{1}{q}}\right)$; now when e is very small, (q being finite,) f is also very small, and (d) gives $2p=1-f$, or $p=\frac{1}{2}$ by rejecting $\frac{f}{2}$ in comparison with $\frac{1}{2}$; hence when $p=\frac{1}{2}$ (and q not indefinitely small) we get for the integral of (1), (observing that $p=\frac{1}{2}$ gives $c=1$,) $y=\int_0^u dx(a^2-x^2)^{-\frac{1}{2}}\cos.bu^q x\left(\mathrm{A}'+\mathrm{B}'\log.u(a^2-x^2)^{\frac{1}{q}}\right)$, (e), A', B' being the two arbitrary constants which (1), an equation of the second order of differentials, requires that the complete value of y should have.

We may observe that Lacroix's integral will always satisfy (1) when p is positive and not indefinitely small, but it will not satisfy (1) when p is negative; also our integral will always satisfy (1) when p is negative, (whether it is indefinitely small or not;) but when p is positive and greater than unity it fails to satisfy it. Again, if we put $c=0$, we get by (a) $p=\frac{q-1}{2q}$, and $\frac{1-q}{2q}=-p$, and the particular value of y which we have found, becomes $y=u\int_0^u dv(a^2-v^2)^{\frac{1-q}{2q}}\cos.bu^q v$, and if we put $b=\frac{1}{q}$ it will become $y=u\int_0^u dv(a^2-v^2)^{\frac{1-q}{2q}}\cos.\frac{1}{q}u^q v$, or if we put $\frac{1-q}{2q}=i$, we get $1-q=2iq\ \therefore q=\frac{1}{2i+1}$, and $2q-2=-\frac{4i}{2i+1}$, and $y=u\int_0^u dv(a^2-v^2)^i\cos.(2i+1)u^{\frac{1}{2i+1}}v$, and (1) becomes $\frac{d^2y}{du^2}+a^2yu^{\frac{-4i}{2i+1}}=0$, the value of y being a particular solution of it; it would now be easy after the manner of Lacroix to deduce (from what has here been done) the second class of the cases of integrability of the equation of Riccati; but as it is sufficiently obvious, we shall

not stop to give it, but shall refer for the process to the same volume and page of Lacroix's work that we have done at the commencement of this paper.

We will now show how to find the integral of (2). If we change b into $b\sqrt{-1}$, in (b), we shall have the complete value of y in the general case when p is positive and less than unity; and if $p=\frac{1}{2}$, $c=1$, by making the same change in (e) we shall have the complete value of y in this case of the general integral.

If p is positive and greater than unity, we must change b into $b\sqrt{-1}$ in Lacroix's integral, and change $-v^2$ in ours into $+v^2$, then using A and B to denote two arbitrary constants, we get $y=A\int_0^{\infty} dx(a^2-x^2)^{p-1}\cos.bu^q x\sqrt{-1}+B\int_0^{\text{infin.}} u^{1-c}dv(a^2+v^2)^{-p}$ cos. $bu^q v$, (f), for the complete value of y. If p is negative and finite, we must change $-x^2$ into $+x^2$ in Lacroix's integral, and b into $b\sqrt{-1}$ in ours, then using A and B for the arbitrary constants we shall have $y=A\int_0^{\text{inf.}} dx(a^2+x^2)^{p-1}\cos.bu^q x+Bu^{1-c}$ $\int_0^{\infty} dv(a^2-v^2)^{-p}\cos.bu^q v\sqrt{-1}$, ($g$), for the complete value of y. We will now give some applications of what has been done to differential equations which can be reduced to the form of (2).

Suppose that we would integrate the equations $\frac{d^2y}{dx^2}+\frac{A}{x}\frac{dy}{dx}-B^2x^ny=0$, ($h$), and $\frac{d^2y}{dx^2}+A\frac{dy}{dx}-B^2e^{nx}y=0$, ($i$), in which x and y are the only variables, y being considered as a function of x, x being considered as the independent variable whose differential dx is supposed to be constant, and e is supposed to be the base of hyperbolic logarithms; if we change the independent variable from x to u, we must not consider dx to be constant, but we may suppose du to be constant; also in (h) and (i) we must suppose that $\frac{d^2y}{dx^2}$ is changed to $\frac{d\left(\frac{dy}{dx}\right)}{dx}$. We shall now put $u=x^{n+2}$, $n+2=m$, and $du=$ constant in (h), then since y is regarded as a function of u, and u of x, we get by well known formulæ of differentiation $\frac{dy}{dx}=\frac{dy}{du}\frac{du}{dx}=m\frac{dy}{du}x^{m-1}$, and hence $\frac{d\left(\frac{dy}{dx}\right)}{dx}=m$

$(m-1)x^{m-2}\dfrac{dy}{du}+m^2\dfrac{d^2y}{du^2}x^{2m-2}$, or since $u=x^m$, we get $\dfrac{dy}{dx}=\dfrac{mu}{x}\dfrac{dy}{du}$, and $\dfrac{d\left(\dfrac{dy}{dx}\right)}{dx}=m(m-1)\dfrac{u}{x^2}\dfrac{dy}{du}+\dfrac{m^2u^2}{x^2}\dfrac{d^2y}{du^2}$; by substituting these values of $\dfrac{dy}{dx}$, $\dfrac{d\left(\dfrac{dy}{dx}\right)}{dx}$, and $x^m=\dfrac{u}{x^2}$ in (h) we get by a slight reduction $\dfrac{d^2y}{du^2}+\left(1+\dfrac{A-1}{m}\right)\dfrac{dy}{udu}-\dfrac{B^2}{m^2}\dfrac{y}{u}=0$, (k), which is reduced to the form of (2) by putting $1+\dfrac{A-1}{m}=2pq-q+1=c$, $b^2=\dfrac{B^2}{m^2}$, $2q-2=-1$, or $q=\frac{1}{2}$; $\therefore p=\frac{1}{2}+\dfrac{A-1}{m}$, also $a^2=1$: hence the integral of (k) is found the same way as that of (2) given above; hence we have y expressed by a known function of u, then putting for u its value x^m, we have y expressed by a known function of x as required. Again, if we put $u=e^{nx}$, we get $\dfrac{dy}{dx}=\dfrac{dy}{du}\dfrac{du}{dx}=n\dfrac{dy}{du}e^{nx}=nu\dfrac{dy}{du}$, and $\dfrac{d\left(\dfrac{dy}{dx}\right)}{dx}=n^2u^2\dfrac{d^2y}{du^2}+n^2u\dfrac{dy}{du}$, substituting these values and $u=e^{nx}$ in (i), we have by a small reduction $\dfrac{d^2y}{du^2}+\left(1+\dfrac{A}{n}\right)\dfrac{dy}{udu}-\dfrac{B^2}{n^2}\dfrac{y}{u}=0$, (l); hence if we put $2pq-q+1=1+\dfrac{A}{n}$, $a^2=1$, $b^2=\dfrac{B^2}{n^2}$, $2q-2=-1$ or $q=\frac{1}{2}$, we shall have $p=\frac{1}{2}+\dfrac{A}{n}$; then the value of y is found in terms of u, as in integrating (2), and by putting for u its value e^{nx}, y becomes known in terms of x as required. The equations (h) and (i) were proposed in the Mathematical Miscellany by Prof. Peirce, at p. 399, first volume; we gave an answer to them in No. 8 of that work, which was incorrect in several particulars, which we shall not stop to notice any further than to observe, that u, the independent variable, when integrating with reference to x in Lacroix's integral, and v of our own, was involved in one of the limits of the integrals, so as to be a function of x or v, which ought not to have been so, but the error was not noticed by us in time sufficient for correction before the solution was published.

We will now reduce the equation $u\frac{d^2z}{du^2}+\frac{dz}{du}-2a'uz-\frac{2u}{b'}$, (m), to the form of (2); (m) is easily changed to $\frac{d^2z}{du^2}+\frac{dz}{udu}-2a'\left(z+\frac{1}{a'b'}\right)=0$, and if we put $y=z+\frac{1}{a'b'}$ we have $\frac{d^2y}{du^2}+\frac{dy}{udu}-2a'y=0$, (n). If we put $2q-2=0$ or $q=1$, $a^2=1$, $b^2=2a'$, $c=1$ or $2qp-q+1=2p=1$, we get $p=\frac{1}{2}$, and (2) will become the same as (n). Hence if in (e) we put $q=1$, $a^2=1$, $b=\sqrt{-2a'}$, we have $y=A'\int_0^1 dx(1-x^2)^{-\frac{1}{2}}\cos.ux\sqrt{-2a'}+B'\int_0^1 dx(1-x^2)^{-\frac{1}{2}}\log.[u(1-x^2)]\cos.ux\sqrt{-2a'}$, (o), for the complete value of y, which is the integral of (n). If we use e for the hyperbolic base, we get by known formulæ $\cos.ux\sqrt{-2a'}=\frac{e^{-ux\sqrt{2a'}}+e^{ux\sqrt{2a'}}}{2}$, and if we put $x=\cos.\varphi$ we get $dx(1-x^2)^{-\frac{1}{2}}=-d\varphi$, $1-x^2=\sin.^2\varphi$, $\cos.ux\sqrt{-2a'}=\frac{e^{-u\sqrt{2a'}.\cos.\varphi}+e^{u\sqrt{2a'}.\cos.\varphi}}{2}$; by substituting these values in (o) we have $y=-\int_0^1 d\varphi\left(e^{-u\sqrt{2a'}.\cos.\varphi}+e^{u\sqrt{2a'}.\cos.\varphi}\right).\left(\frac{A'}{2}+\frac{B'}{2}\log.u\sin.^2\varphi\right)$, the integral being taken from $\cos.\varphi=0$ to $\cos.\varphi=1$; or if we denote $\frac{A'}{2}$ and $\frac{B'}{2}$, by A and B, we have by interchanging the limits of the integral and changing its sign, (which makes no alteration in its value,) $y=\int_1^0 d\varphi\left(e^{-u\sqrt{2a'}.\cos.\varphi}+e^{u\sqrt{2a'}.\cos.\varphi}\right).\left(A+B\log.u\sin.^2\varphi\right)$, the integral being taken from $\cos.\varphi=1$ to $\cos.\varphi=0$, or if we put $\pi=3.14159$, &c. (= the semi-circumference of a circle whose radius $=1$,) we get $y=\int_0^{\frac{\pi}{2}} d\varphi\left(e^{-u\sqrt{2a'}.\cos.\varphi}+e^{u\sqrt{2a'}.\cos.\varphi}\right).\left(A+B\log.u\sin.^2\varphi\right)$, 0 and $\frac{\pi}{2}$ denoting the limits of φ; or (since $\cos.\varphi$ becomes negative in the second quadrant) we shall obtain the same value of y by putting $y=\int_0^{\pi} d\varphi e^{u\sqrt{2a'}.\cos.\varphi}\left(A+B\log.u\sin.^2\varphi\right)$, (m'), the integral being taken from $\varphi=0$, to $\varphi=\pi$, (m') will be found on trial to satisfy (n), and since it involves the two independent arbitrary constants, y is the complete integral of (n) as required.

Since $z+\frac{1}{a'b'}=y$, we get $z=y-\frac{1}{a'b'}=\int_0^\pi d\varphi\left(Ae^{u\sqrt{2a'}.\cos.\varphi}-\frac{1}{a'b'\pi}\right)$ $+B\int_0^\pi d\varphi e^{u\sqrt{2a'}.\cos.\varphi}\left(\log.u+2\log.\sin.\varphi\right)$, or if we still denote $Aa'b'\pi$ (for brevity) by A, $z=\frac{1}{a'b'\pi}\int_0^\pi d\varphi\left(Ae^{u\sqrt{2a'}.\cos.\varphi}-1\right)+B$ $\int_0^\pi d\varphi\left(\log.u+2\log.\sin.\varphi\right).e^{u\sqrt{2a'}.\cos.\varphi}$, (p). If we put $B=0$ $A=1$, we get $z=\frac{1}{a'b'\pi}\int_0^\pi d\varphi\left(e^{u\sqrt{2a'}.\cos.\varphi}-1\right)$, which is a particular value of z that satisfies (m), and agrees with the value used by Laplace in the supplement to the 10th book of the Mécanique Céleste, Vol. IV, p. 60, (of the supplement,) and he expresses it as his opinion that the complete value of z cannot be found by any of the known methods; we will add that the same particular value is to be found in the profound commentary by Dr. Bowditch, at p. 973, Vol. IV.

Again, if $p=0$, (or is indefinitely small,) (2) becomes $\frac{d^2y}{du^2}+$ $\frac{1-q}{u}\frac{dy}{du}-q^2a^2b^2u^{2q-2}y=0$, (q), which is not satisfied by Lacroix's integral, but our integral $u^{1-c}\int_0^u dv\,(a^2-v^2)^{-p}\cos.bu^qv$ $\sqrt{-1}$, which (since $1-c=q$, $(a^2-v^2)^{-p}=1$, rejecting indefinitely small quantities,) becomes $\int_0^u u^q dv\cos.\,bu^qv\sqrt{-1}=$ $\frac{\sin.abu^q\sqrt{-1}}{b\sqrt{-1}}=\frac{e^{-abu^q}-e^{abu^q}}{-2b}=\frac{e^{abu^q}}{2b}-\frac{e^{-abu^q}}{2b}$, (where $e=$ the hyperbolic base,) will satisfy it; it is also evident that (q) ought to be satisfied by each of the values $y=e^{abu^q}$, $y=e^{-abu^q}$, which on trial will each be found to satisfy (q), hence if A and B denote two arbitrary constants, the complete value of y that satisfies (q) is $y=Ae^{abu^q}+Be^{-abu^q}$.

Also, if $p=1$, (or if $1-p$ is indefinitely small,) (2) becomes $\frac{d^2y}{du^2}+\frac{1+q}{u}\frac{dy}{du}-q^2a^2b^2u^{2q-2}y=0$, (r), which is not satisfied by our integral, but Lacroix's integral will satisfy it, that is, (since $(a^2-x^2)^{p-1}=1$,) $y=\int_0^u dx\cos.\,bu^qx\sqrt{-1}=\frac{e^{abu^q}}{2u^qb}-\frac{e^{-abu^q}}{2u^qb}$, will

satisfy it; $\therefore$ the values $y=u^{-q}e^{abu^q}$, $y=u^{-q}e^{-abu^q}$, will each satisfy (r), and if A and B denote two arbitrary constants, $y=\left(Ae^{abu^q}+Be^{-abu^q}\right)\times u^{-q}$ is the complete value of y that satisfies (r). Thus far q has been supposed to be different from zero; but if $q=0$, (2) becomes $\frac{d^2y}{du^2}+\frac{dy}{udu}=0$ or $ud^2y+dydu=0$, $\therefore$ $udy=Adu$, or $dy=\frac{Adu}{u}$, or $y=Ah.l.u+B$, where A and B are the two arbitrary constants which the integral requires. Thus we believe we have integrated (2) completely in all the cases which can occur.

ART. IX.—*Notice of the Zoological Writings of the late C. S. Rafinesque;* by S. S. HALDEMAN.*

CONSTANTINE SAMUEL RAFINESQUE, was born "at Galata, a suburb of Constantinople," October 22d, 1783, and died in Philadelphia on the 18th of September, 1840, of cancer of the stomach and liver. While yet an infant, his parents removed to Marseilles, where he remained some years, previous to being removed to Leghorn. It is apparent throughout his works, that he considers himself a great traveller; thus the motto to his "Life of Travels" is

> "Un voyageur dès le berceau,
> Je le serais jusqu'au tombeau."

He states that his parents took him to Asia while he was an infant, that he *saw* the coast of Africa, and names the places he *would have seen*, if he had been allowed to accompany his father to Canton! According to his own account, he commenced the study of natural history at an early age, which is certainly the fact, as his "description of four new species of birds from Java," (seen in the Philadelphia museum,) was published in the Bul. des Sciences in 1803, when he was but nineteen years old; and his Florula Columbica and Delawarica, were presented to Dr. Barton, for insertion in his Med. and Phys. Journal, the year following.

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* A notice of the Botanical Writings of Rafinesque, appeared in this Journal, Vol. xl, p. 221, April, 1841.

Rafinesque was a most industrious man, and passed a great deal of work through his hands, relating to almost every subject. His life was made up of a series of vicissitudes, and his efforts were retarded by poverty, and the consequent necessity of making a living. His greatest fault as a naturalist was not so much, perhaps, the shortness and resulting obscurity of his characters, as his passion for 'new species,' and the recklessness with which he proposed them, without sufficiently examining the works of his predecessors. The author who pursues such a course, treats his fellow-laborers with disrespect, and prevents his works from being as much consulted as they may deserve; for there is nothing to compel other authors to wade through unsatisfactory descriptions, which must, in many instances, be referred to established species. Rafinesque was very credulous, which led him to believe the exaggerated accounts of the vulgar; and to write essays and found 'species,' upon grounds which should be beneath the notice of any naturalist.

In giving a list of his zoological works, it is more with a view to point them out for the use of those who follow him in the various branches upon which he touched, than to write a critique upon the whole, as this would be impossible; and nothing would be gained by it, as each department must eventually be consulted by those interested. We notice them as nearly as we are able, in chronological order, and believe the omissions will be but few.

1810. *Caratteri di alcuni nuovi generi e nuove specie di anamali, &c.*, 8vo. pp. 105. This work is principally devoted to fish, illustrated by many rude figures, upon seventeen quarto copperplates. It is a good work. Thirty pages, and three plates, are devoted to botany.

Indice D'Ittiologia, &c., 8vo. pp. 70, and two plates. This work contains about three hundred and ninety species of Sicilian fishes, (one hundred and ninety of which are marked as new,) and twenty eight new genera. It is noticed at length by Swainson, in *Fishes*, &c. Vol. I, pp. 60–3 of the Cabinet Cyclopedia. Mr. S., who spent several years in Sicily, states that Rafinesque anticipated many of the genera of Cuvier, and thinks most of his species will yet be brought to light, he having identified many of them himself.

1811. *Description of two new genera of Crustacea, and a new species of Atlantic fish.* Sent to the Lin. Soc.

Zoologie Sicilienne, &c. Containing about three hundred and sixty new species, independent of those already published. Unpublished?

1814. *Précis des découvertes, &c.* This pamphlet contains many descriptions of new animals, commencing with two new genera of bats, the first of which he calls Cephalotes, which contains a new species; and the Vespertilio cephalotes of Pallas, or C. Pallasi, Raf. Geoffroy had previously formed a genus Cephalotes and called this species C. Pallasii. The characters of Geoffroy's genus require *incisors* $\frac{4}{6}$; and Rafinesque's $\frac{2}{6}$, the number in C. Pallasii; which is referred improperly, to his genus. Still Rafinesque's genus is not new, it having been previously characterized as a new genus of Illiger, under the name Harpya, which name (under the Greek form) has been subsequently applied to a genus of birds by Cuvier. Genus ii, Atalapha, Raf. has $\frac{2}{6}$ incisors, and besides a new Sicilian species, he cites the Vespertilio Noveboracensis as A. Americana. He says of his species, Nos. 3 and 4, "j'ai changé le nom trop court et équivoque de *Mus* en *Musculus!*" This change is very unexpected from an author who has done so much in abbreviating names. Genus iii, a Mediterranean cetacean, not noticed by subsequent writers, is considered doubtful. Oxypterus, Raf., was by many considered an imaginary genus, until a second species was discovered by Quoy and Gaimard. Sp. 6 and 7, Gerbillus soricinus and Talpa cupreata, observed in North America. Of five new species of American fish, Centropomus albus, is perhaps the Labrax mucronatus; C. luteus, Perca flavescens; and Sparus mocasinus, Pomotis vulgaris, Cuv.; a Linnean species.* Rafinesque remarks of the *Crustacea*, that "after the fishes, it is in this class that I have made the most numerous discoveries in Sicily; of about one hundred and eighty species that I have observed here, nearly the half are new; they will be all figured and described in my Sicilian Plaxology;" and of the insects, "my discoveries in this class are less numerous; I have about twenty new species." Besides a new genus, the species described are four of Lepisma, two Acari, a Formica, and two Aphides. We cite these to fortify our opinion that Rafinesque had little or no knowledge of Entomolo-

* Among the Sicilian fish is one named Esox reticulatus, a name subsequently applied by Lesueur to a well known American species.

gy; as a great part of his American new species belong to these genera, and to Julus; genera with which every one is familiar. The remainder of the "*Précis*" is taken up (except the botanical portion) with new species of Cephalopoda, worms, and zoophytes.

Principes fondamentaux de Somiologie, ou les Lois de la Nomenclateur, etc. "The *Laws* are necessarily familiar to all professed naturalists; but we have never before met with so welcome a digest of them. Somiology is designed to express the science of organized bodies in one word, and seems derived from *sōma*, a body, and *logos*, a discourse; and, without it, two must be used, as Zoology, and Phytology or Botany."* French is recommended as the language of Natural History, instead of Latin. Another rule should have been added, viz. when a new species is characterized, which has nothing to enable one to recognize the description as belonging to a distinct species, it becomes necessary to state wherein it *differs* from an allied and well known species.

Specchio delle Scienze, &c., 2 v. 8vo., Palermo, 1814. This work was published monthly for one year, when it was discontinued, for the want of sufficient support; a fate which has befallen all the periodical works of this author. He even states that the last number was detained by the printer, although indebted to him, but he must afterwards have succeeded in getting it, as we possess it. There are a number of zoological articles in it, among which are descriptions of two new genera of fish, Leptopus and Nemochirus. *Osservazioni microscopiche*,† are principally devoted to new species of Infusoria. An article on the Sicilian Phocidæ, gives five species under four genera, retaining the Linnean name for "P. vitulina," under which name several species have been

* Loudon's Mag. V, 76. These laws are *not* well known, or we would not have so many barbarous names imposed upon the science from day to day. Our author was particularly happy in his nomenclature, for which he deserves the gratitude of all naturalists. Barbarous names, he says, should be expunged; such as *Messerschmidia, Hoffmansegga, Krascheninikofia, etc.* We protest, however, against the injustice of crediting a genus to an author who has merely varied a name from its original form; as Lepidosteus, *Agass.*, instead of *Lacépède*, who named this genus Lepisosteus. If Agassiz is to have the *genus*, the *species* follow of course; and a rule which leads to such a result, must be false.

† *Arthrodia*, a new genus of Conferva, seems to be identical with Oscillatoria. We do not pretend to determine the right of priority, between Rafinesque and other authors, in the instances cited in this article.

confounded. Aglophema, Raf. has precedence of F. Cuvier's Arctocephalus; Aglophema pusilla (Phoca pusilla, Lin.) being the type of both; Rafinesque, however, calls it A. phoca, changing the specific name without cause. Authors are not agreed upon the habitat of this species, but its range may be extensive, or distinct species may be confounded under the same name.

The "*Prodromus of Sicilian Herpetology*," suggests such 'improvements' as Batrachus, for Bufo; Ranaria, for Rana; and Hylaria, for Hyla! and contains notes of a considerable number of new species.

Thirty six new genera of Marine animals of Sicily. Of five mollusks, Oxynoe is said to differ from Sigaretus, by having an external shell;* and of three Limaces, one (Tylodina) appears to be identical with Testacellus. Artedia differs from Planaria by having two little tentacles. As we will have occasion to refer to his section of Porostomes, we give its characters here—Porostomi-*Animali senza bocca apparente, e nutrendosi con pori superficiali; quasi sempre gelatinosi e natanti.* Vol 2, p. 165.

1815. *Description of Balæna gastrytis*, a new Mediterranean species. In the Palermo Port Folio.

Analyse de la Nature, &c., pp. 224, 8vo., with a portrait. Among his manuscripts, I find a sketch of his life written as by another person, wherein it is stated that he esteemed this work more than any other of which he is the author. We cite his orders of Mammalia, the three first of which form his sub-class Chiropodea. 1. Primatia, 2. Chiropteria; 3. Exogenea (Marsupials); 4. Stereoplia (Solipeda, Ruminantia); 5. Pachydermia; 6. Anodonea (Edentata); 7. Gliria; 8. Ferea; 9. Amphibia (Phoca, Manatus); 10. Cetacea.

1816. *Observations on the Sturgeons of N. America*, and description of a new species, Accipenser marginatus, for the Phil. Soc. of Philadelphia.

Circular address on Botany and Zoology, &c. This is a general invitation to all to forward the author specimens, &c. for exchange, as "rare pamphlets, and publications not in my possession..... I shall receive thankfully any kind of information or news relating to natural sciences, such as discoveries, publications, proceedings of individuals and learned societies, &c. I particu-

* See the note to Pl. 49 of Deshayes' edition of the Règne Animal.

larly beg for complete sets of the natural orders and families of orchideous, ombelliferous, liliaceous, grasses, mosses, lichens, marine plants, labiated, leguminous, &c., and for specimens, or the characters of all the new genera..... I beg the coöperation of all my friends and correspondents; inviting them to communicate every particular, even of the most trifling nature, or obsolete, relating to the properties, qualities, uses, employments, &c., of all plants and animals; provided they are unpublished, else it will be sufficient to let me know (or send me if rare) in which works or pamphlets they are already published."

These extracts show that our author was preparing to get as many materials from every quarter as possible, evidently with the intention of getting new genera, species, and observations, from the labors of others. That he had an especial interest in getting assistance from all parts, is evident from the avidity with which he attacked every subject.*

There are two prizes offered on the cover of the Atlantic Journal, which place the intention of these requests in a still stronger light. They are, "*Fifty dollars* in books for the best memoir on the effects on the earth and mankind of the geological flood or floods, all over the globe as far as known; including *all accounts without exception*, preserved by history or traditions, among all *the nations* of the earth. *Twenty five dollars* in botanical books and herbariums, to the author of the best synopsis of *all* native *phenogamous plants* of the U. S. as far as known; provided that not a single one already described or published in America or Europe be omitted!!"

The absurdity of these *prizes* is sufficient to make one doubt the sanity of the man; for who could be induced to write a synopsis of American plants for *such* a reward! Judging from the appearance of the specimens, his method of preserving plants was more simple than any recommended in books, as it consisted in placing

* Thus among his MSS. lost by shipwreck are the following, most of which he intended to re-write! A greater piece of presumption than this list indicates, cannot be cited, when we consider the talents and the means of the man. His industrious habits would never have compensated for his extreme carelessness and want of method, and his poverty prevented him from obtaining the requisite works, his library containing scarcely any thing modern.—*Critiques des genres, &c.* An investigation of all generic names of plants and animals.—*Amenities.* Nearly one hundred tracts on Zoology and Botany.—*Fauna Sicula*, with nearly four thousand species.—*Genera of Birds*, with many new genera.—*Synopsis* of all known species of quadrupeds and fishes.

the newly gathered examples between paper (without pressure) where they were left without being disturbed, until required.*

The circular contains a prospectus of a Flora and Fauna of N. America, in which he proposes to figure every animal and plant on wood, and that every one may have such a portion as he may require, partial sets are indicated as a fauna and flora ornata, economica, dietetica, Virginica, &c. &c., amounting to no less than one hundred and fifteen varieties of flora, and nearly as many of fauna.

1817–18. *American Monthly Magazine.* This periodical contains many descriptions of bats, reptiles, fish, crustacea, &c. and Notrema, a curious genus of shell from the Ohio, which resembles Fissurella. The name is changed to Tremesia in the Monograph of Ohio shells. This animal is said to have been discovered by Audubon, and communicated to Rafinesque, who described and figured, without having seen it. It certainly cannot be admitted into the systems of malacology without further investigation. His Mazama (Ovis montana, Ord.) is identical with Aplocerus of Smith, and was probably first published.† If this is the case, Smith's genus Mazama is left without a name. Many new species of Aphis, (and two new genera,) are described in this periodical; and, in the extended article "on water snakes, several species of Sea-serpent are named, principally from newspaper paragraphs. See a list of these papers, appended to the Florula Ludoviciana.

1819. *Seventy new genera of animals* in the Journal de Physique, Vol. 88. This paper is too long for analysis. Two Cheiroptera are noticed, and some "genera" of Helix proposed. The fishes described are reproduced in his Ohio fishes; and many of the genera, especially among the zoophytes, are fossil.

Several genera and species of fish are described in the Jour. Acad. Nat. Sci. Vol. I, and in the first volume of the American

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* The greater part of his fossils resembled his plants, as any stone which was marked with the slightest ridge or furrow, or bore any vestige of organic remain, was carefully preserved, together with strange looking pebbles and waterworn fragments. *Bushels* of such trash were sold at the sale of his effects, for trifling sums, but the specimens were absolutely worthless; the localities even being unknown. There were many bad specimens of Unio, mostly odd valves, among them.

† It includes Smith's genus Subulo! one species being called Mazama pita, and another M. bira. These names are taken from Azara. See Hunter's trans. vol. I, p. 141 and 145.

Journal of Science.* Of these we are acquainted with Exoglossum alone, which is founded upon natural characters, and is a good genus.

Without knowing their precise date, we will here notice a series of articles from vols. 5, 6, and 7 of the An. Gen. des Sci. Phys., published at Brussels. Ours is a detached set, and we may therefore have omitted other articles from the same work.

Prodrome d'une Monographie des Turbinolies du Kentuky, par C. S. Rafinesque et J. D. Clifford. Five sub-genera and sixteen species are described.

Monographie des coquilles bivalves et fluviatiles de la rivière Ohio, (with figures.) As Rafinesque was the first to make known the greater part of the western Unios, it is but fair that those who study this genus, should exert themselves to identify his species. They are surrounded by fewer difficulties than those of Linnæus, yet there is little doubt respecting the latter, and as Rafinesque sold examples of his species to any one disposed to purchase, he certainly must be credited with the disposition to afford every assistance. The most complete collection of authentic specimens now existing, is that of Mr. Poulson† of Philadelphia, who also possesses many of Rafinesque's unpublished MSS. and drawings.‡ Most of these species are, in fact, so well established, that it is a mere affectation to assert that they cannot be identified. The greater part of Mr. Swainson's sub-genera of American Unios in the Cabinet Cyclopedia, were previously indicated by our author. The same paper contains a division of the genus Cyclas into subgenera, but without any notice of Pisidium.

Sur les animaux polistomes et porostomes. The former are Zoophytes, the latter Infusoria, which with the older authors, he supposed to take their nourishment by means of pores, whence the name. As an example of the style, we transcribe a few of the introductory observations. "Des erreurs accréditées pas des savans illustres, admises tacitement comme des vérités démontrées par la foule des copistes et des esprits superficiels qui se contentent de croire sur parole, sont bien difficiles à détruire; néam-

* Several reptiles are described here, also.

† Our cabinet contains three shells not in Mr. Conrad's list, viz. Unio pallens, metaplatos and bicolor; the last is a variety of U. dilatatus, Raf. Mr. P. is so liberal that he gives every facility to those who wish to consult his fine collection and library.

‡ We have a considerable number of these.

moins il est du devoir de ceux qui ont verifié et constaté les faits réels qui les détruisent, de chercher à les divulger et à éclairer la domaine des sciences..... Il est plus facile de copies des erreurs, que de rechercher la vérité, et quand elle est découverte, elle a souvent bien de la peine à percer les nuages de l'ignorance ou des préjugés scientifiques."

Remarques sur trois erreurs ichthyologiques. "The first is the absurd division of fishes into osseous and cartilaginous;" the second, that authors consider the Pleuronectes as thoracic instead of jugular; and the third, that the prepared fish roe called botargo, does not belong to the Mugil, but to the Tunny.

Sur quelques animaux hybrides. This apocryphal account, (founded upon hearsay,) relates to such animals as Felis domestica, Didelphis Virginianus; and Procyon Vulpes.

Sur le genre Manis, et description d'une nouvelle espèce. Three species (two of which are Linnean) are described under two subgenera, which, with changing specific names, has enabled our author to append his name to them all!

Western Review. Several articles are inserted here, but we can only mention the *Canis leucurus*, a white tailed fox of Kentucky.

1819. *Twenty four lectures on the natural history of the Universe, the earth and mankind, animals and plants.* (MS.) These unpublished lectures are in our possession; they treat of astronomy, meteorology, geology, mineralogy, crystallography, &c. Nine of them constitute the zoological portion, and indicate but little talent. The introductories are good, and those devoted to American geology amusing, from the singularity of the views advanced.

1820. *Ichthyologia Ohiensis*, one vol. 8vo., pp. 90. One hundred and eleven species are described. It is a valuable contribution to this branch of science, and Prof. Kirtland's labors in the same field will render a particular notice unnecessary. He very properly separates the broad-mouthed, from the narrow-mouthed Lepidostei.

Fishes of the Susquehanna. (Unpublished MS.) The descriptions are too short to enable one to make out all the species; and, as usual with our author, species are multiplied on the strength of the locality. "Perca interrupta, Raf." is Labrax lineatus, Lin.; Esox chlorops = reticulatus, Les.; and Luxilus verrucosus, is probably Cyprinus cornutus, Mitch. Thirty seven species are described, and thirteen are certainly omitted, which gives fifty species to the Susquehanna. Among the omissions

are all the cartilaginous fishes: viz. Sturio; Petromyzon Americanus, Les.; P. (Bdellostoma) nigricans, Les.; and Ammocætes bicolor, Les. Lepisosteus osseus, Lin. is omitted also.

Annals of Nature. This tract contains eighty one new species and a proportionate number of new genera of animals. The first twelve are "Mastosia or Sucklers," including three bats, a Mephitis, (probably the common species, which varies much,) a species of Spalax, two of Gerbillus, three of Lemmus, and a Sciurus. Of four birds, Milvus leucomelas appears to be Nauclerus furcatus, Lin. Hirundo phenicephala, (head scarlet,) is given on the authority of Mr. Audubon, who, however, does not describe it. The first reptile described is a species of *Necturus*, a genus proposed for the Salamandra Alleganiensis, afterward described under a new generic name by Dr. Harlan. Several species of *Triturus* follow, this name being applied to the reptile Triton, (there being a mollusc Triton,) because the two are inadmissible. The reptile has precedence in point of time, (at least, this is our impression,) and Laurenti* could not have been, under ordinary circumstances, deprived of his genus, merely because Lamarck thought proper to adopt the name for a different one. Many of the serpents named in this pamphlet, are evidently described from hearsay.

1831. *Enumeration and account of some remarkable natural objects, &c.* This tract is chiefly devoted to fossils, and is partly a catalogue of objects which he had for sale. His Mazama salinaria is minutely characterized from a horn. This 'unique specimen of great beauty and value,' is in our cabinet; but those who wish to possess so desirable an object, can be gratified, if they will take the trouble to break the prongs from the horns of a Cervus Canadensis, and deposit them where the inside porous portion may become filled with mineral matter.

1832–3. *Atlantic Journal.* This periodical is principally filled with the productions of the editor, although sometimes over fictitious signatures, which can never mislead any one acquainted with his style. Art. 13, on the Mexican Jaguars. He cites several instances in which these animals have penetrated into the Western States. Art. 14, Cougars of Oregon. He acknowledg-

* According to Cuvier, Laurenti's name was applied to a larva; consequently it cannot be retained, and that of Rafinesque must stand. See Harlan's Researches, p. 165, note *.

es several *varieties*, but contrives to make species out of them thus: Var. Oregonensis, dark brown, &c..... Is it not a peculiar species? *Felis Oregonensis.*—I find in Leraye's travels,* that a smaller animal nearly similar in color, but not longer than a cat, is found, &c. Is it a new species? *Felis macrura*, Raf. Art. 10, Aquila dicronyx, Raf., appears to be identical with Holiaetus, Washingtoni, Aud. The old genus Condylura of Illiger, is reproduced under the name Astromycter. We have seen Rafinesque's Psephides, "a new tubular fresh-water shell of the Allegany mountains," and consider it the case of a larva. Whether the many species of fossil shells which Rafinesque has described, from the Alleganies of Pennsylvania, are new, will of course be determined by the geological survey of this State, (Pennsylvania,) which is drawing to a close.

1836. *A life of travels and researches, &c.* We close this article with a few extracts from this work.

"Mr. Gibbs, consul of the U. S., received me well; he was also a banker and merchant. I became his secretary and chancellor. I dwelt with him in a palace till 1808, when I took a house of my own and became a merchant, having made a small fortune in his employ within three years. Shortly after my arrival, political events made Sicily the residence of the court of Naples, and broke all our communications with Italy and France. The produces of the island fell to a low rate: it was by trading in them that I acquired my first personal fortune, as well as by discovering in the island several new drugs and sources of trade. Such were the squills, rosemary, wormwood, bay-leaves, &c. I established a manufacture of dry squills on a large scale: the Sicilians were wondering at me for this, as they made no use of them, and fancied they were a new tinctorial article, which I let them believe.... I wrote in Italian through prudence, rather than in French. Prudent considerations had already induced me to add the name of *Schmaltz*, my mother's name, to my own, and to pass for an American."

"Swainson went often with me in the mountains; he carried a butterfly-net to catch insects, and was taken for a crazy man or a wizard; as he hardly spoke Italian. I had once to save him from

* These travels cannot be received as zoological authority; the species, therefore, that Rafinesque has founded upon them, (Am. Monthly Mag. v. 1, p. 435,) are not worth looking after.

being stoned out of a field, where he was thought to seek for a treasure buried by the Greeks!....

"Such is then the picture of my life, my labors, and my travels. I give it to the public, or rather to the learned, as an uncommon instance of perseverance and industry. May this inspire youthful minds with a wish to do as well; and the friends of sciences with the wish to know me, or patronize the labors of my old age: permit me at last to produce under their shield, those works, fruits of my travels and researches, which I desire to leave as monuments of my life and exertions.

"If I have often gone beyond the actual state of knowledge in my views and opinions, or anticipated on future knowledge, it was with the noble aim of adding my mite to the mental improvement of mankind. If my discoveries and projects have not been speedily admitted, I leave them as a legacy to those superior minds who will be able to appreciate them, and bestow me the justice often denied in my days: to the friends of useful sciences, of virtue and peace, to the wise philanthropists, to the enlightened, liberal and impartial men of both hemispheres."

Near Marietta, Pa., April, 1842.

ART. X.—*M. Faraday's answer to Dr. Hare's second Letter.**

Royal Institution, Dec. 24, 1841.

My Dear Sir—On reading your second letter to me in Silliman's Journal, (published July, 1841,) I wrote a brief answer back, but find from Dr. Silliman, that it has been mislaid. I therefore send this brief note to say that I hope you will excuse any controversial reply. I do not find any reason to change my opinion as to the matters referred to in yours to me: and as far as I should have occasion to answer for my own part, I would rather refer the readers of the Journal to my papers and my former reply to your first letter. As to the new and important matter into which your last letter would lead me, I am not sufficiently clear in my mind, upon the evidence we as yet have, to wish to enter into it at present. Ever my dear Sir,

With the highest esteem, yours very truly,

M. FARADAY.

Dr. HARE, &c. &c. &c.

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* Communicated from M. Faraday to this Journal.

ART. XI.—*Meteorological Observations, made at the Mines of San Fernando, situated in the Partido de la Manicaraqua, Island of Cuba,* Lat. 22° 20′ 14″ N., Lon. 73° 51′ 27″ W. of Cadiz, at an elevation 554 feet above the sea; by JOHN H. BLAKE, of Boston, Mass.

DATE, 1840.	Barometer.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at 6 P. M.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	REMARKS.
Janu'ry		10.6	67	°	°	82	°	A shower.
2		12.3		68	63			
3		13.5		66	62		74	
4		13.5	61	67	60			
5	29.28	15.5	65	71	65	80	83	
6	29.26	19.	68			81		Drizzling.
7	29.21	19 5	71	76	67	84	84	
8	29.23	8.3	68	74	65	86	116	
9	29.23	21.5	65	72	65	74	99	
10	29.23	18.	66		63			
11	29.10	29.	67	71	67	86	111	
12	29.15	28.	69	73	68	93	115	
13	29.21	27.	68	73	68	89	106	
14	29.22	25.	68	71	70	88	123	
15	29.25	23.	67	72	65	90	113	
16	29.20	21.	60	71	67		109	
17	29.15	21.	68	71	64		107	Hazy.
18	29.20	21.	67	71		85	107	
19	29.20	21.		70	61		85	
20	29.20	21.	67	68	62	71	81	
21	29.25	21.	64	70	69	81	84	
22	29 22	19.	68	74	71	81	106	
23	29.25	21.	71	78	72	91	98	
24	29.15	18.2	69	76	72		97	
25	29 26	16.5	73	77	73	84		
26	29.19	23.5	73	76	72			Thunder storm.
27	29 20	23.5	72	74	68		97	Drizzling.
28	29.23	17.	71	75	70	74	108	Thunder without rain.
29	29.22	13.5	70	75	70		110	A shower.
30	29 10	19.	71	76	69	80	101	A shower.
31	29.28	11 5	74	75	72	97	111	

Barom.	Average for the month,	29.208
	Maximum altitude,	29 28
	Minimum altitude,	29.10
Hygrom.	Average for the month,	19.1
	Extreme dryness,	29.
	Extreme moisture,	8.3
Temp.	Mean for the month,	74.9°
	Maximum in shade,	78.
	Minimum in shade,	60.
	Mean in sun's rays,	92.6
	Maximum in sun's rays,	123.
	Minimum in sun's rays,	80.

RAIN, &c.

Total of water deposited during the month, 2.74 inches.

Greatest quantity that fell in one shower, 1.20 inches.

Number of showers, 4.

Showers with lightning, 1.

Prevailing wind during the month, easterly.

Prevailing clouds, cumulus.

DATE, 1840.	Barometer.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at 6 P. M.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	REMARKS.
Feb.			○	○	○	○	○	
1	29.19	18.5	73	77	73	79	106	
2	29.15	21.	73	77	73	83	125	
3	29.12	18.2	71	76	71		110	Drizzling.
4	29.19	12.	67	67	64			Drizzling.
5	29.22	17.5	62	70	64	65	105	
6	29.22	17.5	71	75	66	95	95	
7	29.37	17.	69	74	69	71	112	
8	29.27	11.	70	74	68	80	80	
9	29.17	17.6	70	74	72	94	110	
10	29.08	17.5	68	75	70		112	
11	29.18	20 3	71	71	60		107	
12	29.23	16.5	66	68	64	70	75	
13	29.27	18.	64	72	67		98	
14	29.30	19.5	70	74	71	87	87	
15	29.22	16.	65	72			82	Drizzling.
16	29.28	14.		71	67		106	
17	29.22	17.	70	72	66	84	114	
18	29.21	17.2	72	87	69	101		
19	29.21	19.	71	74		83	102	
20	29 20	20.5	72	74	68	89	99	
21	29.26	18.4	70	76	67	74	122	
22	29.21	20.5	61	72	67	68	112	
23	29.23	13.2	69	74	74	102	116	Drizzling.
24	29.11	15.	71	74	72	83	123	
25	29.12	20.	71	77	72	72	125	Drizzling.
26	29.23	20.	72	75	73	80	84	Drizzling.
27	29.20	11.	73	76	74	80	96	
28	29.33	9.5	74	78	74	81	92	Thunder storm.
29	29.17	7.8	74	76	68		85	Shower.

Barometer. Average for the month, . . . 29.213
Maximum altitude, . 29.37
Minimum altitude, . 29.11
Hygrometer. Average for the month, . . . 16.5
Extreme dryness, . 21.
Extreme moisture, . 7.8
Temperature. Mean for the month, . . . 70.5°
Maximum in shade, 87.
Minimum in shade, 60.
Mean in sun's rays, 92.2
Maximum in sun's rays, 125.
Minimum in sun's rays, 65.

RAIN, &c.

Total of water deposited during the month, .59 of an inch.

Greatest quantity that fell in one shower, .20 of an inch.

Number of showers, 2.

Showers with lightning, 1.

Prevailing wind during the month, easterly, but variable.

Prevailing clouds, cumulus.

DATE, 1840.	Barometer.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at 6 P. M.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	REMARKS.
March			♀	♀	♀	♀	♀	
1	29.28	9.	74	74	71	80	96	Drizzling.
2	29.15	17.5	71	74	68	80	90	Shower.
3	29.24	9.	70	75	68	92	99	
4	29.01	20.3	71	74	66	74	82	Shower.
5	29.07	21.5	69	74	65	92	122	
6	29.25	12.	68	72	65	92	81	
7	29.20	15.5	69	73	71	86	84	
8	29.22	17.	69	74	69	82	127	
9	29.11	21.5	69	72	71	98	124	
10	29.08	20.	70	76	68	97	100	High wind.
11	29.10	22.8	71	74	74	96	111	
12	29.15	23.	73	77	69	106	117	Thunder storm.
13	29.31	20.5	72	74	65	97	110	
14	29.41	10.5	70	76	67	80	107	
15	29.34	13.5	68	78	71	88	120	
16	29.17	24.	73	79	72	92	106	
17	29.23	10.5	75	79	77	100	138	Showers.
18	29.26	10.8	76	79	72	93	83	Shower.
19	29.22	13.	70	78	73		86	Shower.
20	29 24	10.5		77	72		112	
21	29.23	9.5	75	78	70	91	95	
22	29.21	11.	73		72	84		
23	29.19	10.	73	78	76	82	115	
24	29.13	12.7	74	78	76	81	85	Drizzling.
25	28.98	10.	75	78	74	82		Thunder storm.
26	29.21	12.	65		69	76		
27	29.20	20.	65	70	72	86	106	
28	29.30	14.	69	74		82	84	
29	29.26	11.	73	76	75	80	124	
30	29.24	10.	71		76	82		
31	29.24	10.5	72	76	77	100	120	

Barometer. Average for the month, ... 29.20
Maximum altitude, . 29.41
Minimum altitude, . 28.98
Hygrometer. Average for the month, . . . 14.6
Extreme dryness, . 24.
Extreme moisture, . 9.
Temperature. Mean for the month, . . . 73.5°
Maximum in the shade, 79.
Minimum in the shade, 65.
Mean in sun's rays, . 96.
Maximum in sun's rays, 138.
Minimum in sun's rays, 74.

RAIN, &c.

Total of water deposited during the month, 2.49 inches.

Greatest quantity that fell in one shower, 1.13 inches.

Number of showers, 7.

Showers with lightning, 2.

Prevailing wind during the month, westerly, but very variable.

Prevailing clouds, cirrus.

DATE, 1840.	Barometer.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at 6 P. M.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	REMARKS.
April								
1	29.30	10.	73	77	77	87	98	
2	29.28	11.5	75	79	79	90	114	
3	29.30	11.	77	81	80	101	111	
4	29.31	11.	76	80	80	82	96	
5	29.39	12.		82	82		105	
6	29.30	10.5	77	80	80	88	115	
7	29.28	10.5	76	79	82	90	128	
8	29.23	11.5	70	82	79	78	92	Windy.
9	29.23	10.2	76	79	79	103	116	Rain.
10	29.25	8.5	78	80	76	112	122	Thunder, but no rain.
11	29.23	8.	72	78	77	88	105	
12	29.21	9.	75	79	77	90		Rain.
13	29.25	7.5	75	79	77	97	85	
14	29.21	7.5	75	81		82	100	
15	29.25	7.5			79			
16	29.28	8.		80	79		110	
17	29.23	8.5	76	80	79	93	135	
18	29.28	8.7	78	82	82	92	129	
19	29.33	9.	77	82	82	92	111	
20	29.33	10.5	78	81	81	94	110	
21	29.32	12.	77	81	80	82	117	
22	29.28	13.5	80		79		116	
23	29.27	14.5		81	80		106	
24	29.21	12.		81	79		109	
25	29.19	11.5	71	81	79		113	Rain.
26	29.18	11.5	76	81	79	95	108	Rain.
27	29.22	11.5	76	80	77	92		Thunder storms.
28	29.27	11.5	77	82	80	88	113	Thunder storm.
29	29.34	12.3	80		81			Thunder storm.
30	29.33	12.5		81	82		123	

Barometer. Average for the month, . . . 29.269
Maximum altitude, . 29.39
Minimum altitude, . 29.18
Hygrometer. Average for the month, . . . 10.5
Extreme dryness, . 14.5
Extreme moisture, . 7.5
Temperature. Mean for the month, . . . 78.1°
Maximum in the shade, 82.
Minimum in the shade, 70.
Mean in sun's rays, 101.1
Maximum in sun's rays, 135.
Minimum in sun's rays, 82.

RAIN, &c.

Total of water deposited during the month, 2.09 inches.

Greatest quantity that fell in one shower, 1.35.

Number of showers, 7.

Showers with lightning, 3.

Prevailing wind during the month, northerly.

Prevailing cloud, cirrus.

DATE, 1840.	Barom. at 32 F.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at 6 P. M.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	REMARKS.
May			°	°	°	°	°	
1	29.28	12.	77		82	94		Shower.
2	29.33	12.5			83			Shower.
3	29.22	12 3	79	82	80	102	95	
4	29 22	15.	79	85	80	94	111	Shower.
5	29.18	11.	73	80	79	88	83	
6	29.15	11.	75	82	77	104	99	Shower.
7	29.13	10.5	79	84	82	88	87	Drizzling.
8	29.17	12 5	80	83	79	92	122	
9	29.15	12.5	79	84	82	103	99	
10	29.15	13.	78	82	83	95	113	
11	29.18	13 5	77	82	81	98	101	
12	29.22	12.8	78	82	78	110	122	Shower.
13	29 20	11.	76	80	77	92	121	Shower.
14	29.21	12.7	81	78	75	94	86	Shower.
15	29.14	12.2	74	77		80	109	Shower.
16	29.08	11.5	73			85		Shower morn and eve.
17	29.06	10.		75	74		76	Shower—a rainy day.
18	29.08	10.	74	76	73			Shower—a rainy day.
19	29.08	10.8	73	75				Shower—a rainy day.
20	29.16	8.5	75	79	77	108	106	Shower.
21	29.13	10.5	75	79	79	105	106	
22	29.13	10.	76	78	79	86	104	Shower.
23	29.22	10 2	76	79	77	83		Shower.
24	29.21	9.5	76	79	77	118		Shower.
25	29.16	10.2	76	79	79	109	86	
26	29.13	10.5	76	79	79	85	97	Shower.
27	29.08	11.		80	78	98		
28	29.08	9.8	76	80	78			Shower, and heavy rain at night.
29	29.15	10.	74	77	78		94	Shower, cloudy during the day, but little rain.
30	29.13	10.	76	80	79		82	Showers occasional, and at night heavy rain.
31	29.14	9.3	74	76	76			Shower—a rainy day and night.

Barom. Average for month, 29.159
Maximum altitude, 29.33
Minimum altitude, 29.06
Hygr. Average for month, 11.1
Extreme dryness, 13.
Extreme moisture, 8.5
Temp. Mean for month, 78.°
Maximum in shade, 85.
Minimum in shade, 73.
Mean in sun's rays, 98.
Maximum in sun's rays, 122.
Minimum in sun's rays, 80.

RAIN, &c.

Total of water deposited during the month, 20.28 inches.

Greatest quantity at one time, 7.3 inches.

Number of showery days, 21.

Showers with lightning, 15.

Prevailing wind, easterly, but very variable.

Prevailing clouds, cirrus, cumulo-stratus, and nimbus.

DATE, 1840.	Barometer.	Hygrometer.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at eve.	Temp. F. in sun's rays at morn.	Temp. F. in sun's rays at noon.	REMARKS.
June								
1	29.04	9.	76	79	74	86		A rainy day.
2	29.11	8.4	75	78	77	108		A rainy day.
3	29.21	7.	76	79	78	84	120	
4	29.23	7.5	76	80	80	86	98	
5	29.26	8.5	75	79	76			
6	29.26	9.	73	80		85	118	Slight shower, P. M.
7	29.22	6.	76	76	72			A shower, P. M.
8	29.19	7.	74	78	76	99		Showers.
9	29.19	5.	74	78	77	95		A shower, P. M.
10	29.24	7.5	77	80	80	92	118	
11	29.21	10.	77	80	77	81		A shower, P. M.
12	29.15	8.	76		77	85		
13	29.18	8.9	76	78	72	79	108	A shower, P. M.
14	29.24	8.8	76	79	73			A shower, P. M.
15	29.22	5.7	74	78	73	75		A shower, P. M.
16	29.22	7.8	71	76	72	86		A shower, P. M.
17	29.28	6.5	73	77	76	100	106	
18	29.32	6.8	77	82	78	86	95	A shower, P. M.
19	29.30	6.5	76	82	77	95		
20	29.28	8.6	77		84	97		
21	29.26	9.2	78	91	84	98	92	
22	29.29	9.4	77	90	80	89	103	
23	29.35	9.2	76	90	78		111	
24	29.34	10.		91.5	77			A shower, P. M.
25	29.28	5.	74	90	72		106	A shower, P. M.
26	29.24	10.5	86	87.5	78	110	108	
27	29.24	8.	82	89		90	102	A shower, P. M.
28	29.24	7.	82.5	79.5	76.5	104		A shower, P. M.
29	29.23	8.5	76	88	78	95	128	A shower, P. M.
30	29.22	8.5	77	89	76.5	83		

Barometer. Average for the month, . . . 29.228
Maximum altitude, . 29.35
Minimum altitude, . 29.04
Hygrometer. Average for the month, . . . 7.3
Extreme dryness, . 10.5
Extreme moisture, . 5.
Temperature. Mean for the month, . . . 82.4°
Maximum in the shade, 91.5
Minimum in the shade, 71.
Mean in sun's rays, 99.9
Maximum in sun's rays, 128.
Minimum in sun's rays, 75.

RAIN, &c.

Total of water deposited during the month, 14.56 inches.

Greatest fall of rain in one shower, 2.5 inches.

Number of showers, 17 days.

Showers with lightning, 17 days.

Prevailing wind, easterly.

Prevailing clouds, cumulo-stratus.

MONTHS.	Temp. F. in shade at 8 A. M.	Temp. F. in shade at noon.	Temp. F. in shade at eve.	Temp. F. in sun's rays at 8 A. M.	Temp. F. in sun's rays at noon.	Maximum temp. in shade.	Minimum temp. in shade.	Maximum temp. in sun's rays.	San Fernando, Island of Cuba, Lat. 22° 20′ 14″ N., Lon. 73° 51′ 27″ W. of Cadiz. *Remarks*, 1839.
January. Mean 69.9°	66.9°	71.5°	68.3°	79.7°	95.2°	79°	57°	117°	4 days showers, 1 of heavy rain.
February. Mean 71.4°	68.9	73.	69.8	87.1	98.	78	57	117	6 days showers.
March. Mean 73.2°	73.2	76.6	69.9	90.9	105.8	82	62	135	9 days showers.
April. Mean 74.6°	73.8	78.7	70.6	99.	110.9	83	64	130	6 days showers, 2 of them with lightning.
May. Mean 77.9°	77.	80.6	75.3	101.4	102.1	85	71	119	24 days showers, 15 of them with lightn'g.
June. Mean 78.9°	78.5	82.2	75.7	100.4	111.3	85	72	129	21 days showers, all with lightning.
July. Mean 80.5°	78.5	83.1	78.	101.	106.5	86	71	131	22 days showers, 21 of them with lightn'g.
August. Mean 79.6°	79.7	83.2	76.1	99.6	100.	87	71	120	20 days showers, 15 of them with lightn'g.
September. Mean 78.6°	78.2	82.7	74.6	93.7	96.8	86	68	121	16 days showers, 11 of them with lightn'g.
October. Mean 75.9°	74.9	78.3	73.5	85.	99.7	81	70	110	22 days showers, 1 of them with lightn'g.
November. Mean 72.7°	70.7	75.7	69.8	83.9	96.3	80	65	121	2 days showers.
December. Mean 67.9°	66.8	71.3	64.6	91.9	101.6	77	51	122	6 days showers, 2 of them with lightn'g.

Mean temperature of the spring, . . .	75.2° F.
Mean temperature of the summer, . . .	79.6
Mean temperature of the autumn, . . .	75.7
Mean temperature of the winter, . . .	69.7
Mean temperature of the year, . . .	75.

The greatest variation of temperature in one day during the month of January, was 13°; in February, 13°; in March, 13°; in April, 12°; in May, 13°; in June, 11°; in July, 12°; in August, 13°; in September, 15°; in October, 9°; in November, 11°; in December, 18°.

Note. The degree of humidity of the atmosphere was imperfectly determined by wetting the bulb of a delicate thermometer covered with cambric, with 40 grs. of alcohol, and noting the number of degrees which the mercury fell below the temperature of the air in the shade.

To the very useful averages of the author for the year, we take the liberty of adding the following derived from his records.—Eds.

Average of the maximum temperature in the sun's rays,	122.66°
Average of the temperature at noon in the sun's rays,	110.3
Average of the maximum temperature in the shade, .	82.42
Average of the temperature at noon in the shade, .	78.1
Average of the minimum temperature in the shade, .	64.93
Average of the temperature in the evening, . . .	72.2
Average of the temperature at 8, A. M. in the shade, .	74.
Average of the temperature at 8, A. M. in the sun's rays, .	92.8

Art. XII.—*Reply to Dr. Hare's Objections to the Whirlwind Theory of Storms;* by W. C. Redfield.

An article entitled "*Objections to Mr. Redfield's Theory of Storms, with some strictures on his reasoning;* by Robert Hare, M. D., Prof. of Chem. in the Univ. of Pennsylvania," which appears in the last number of this Journal, and is also found in a modified form in the London, Edinburgh and Dublin Philosophical Magazine for December, has given occasion for the notes and remarks which follow.

The several series of facts and observations, showing both the rotary and progressive movement of great storms, which I have published, together with those which have also been adduced by Reid, Milne, Dové and Piddington,* are deemed sufficient to establish the whirlwind character of these storms. In the absence, therefore, of contravening facts of a reliable character, it seems incumbent on an objector to set aside these facts and observations as unfounded and inaccurate, or to show that the results which they appear to establish have been deduced erroneously. This task Dr. Hare has not attempted; and I might therefore have been excused from replying to his objections and strictures; as these cannot affect the results which it has been my chief aim to establish.

But the observations which I have published extend also to the so-called tornado or water-spout, and with similar results:† while Mr. Espy and Dr. Hare have each in turn advanced his theory of tornadoes and storms, founded on *a priori* reasoning or speculation, and on alleged deductions from phenomena observed. Hence, perhaps, originates this fourth attempt, from one or other of these sources, to discredit the results of my principal inquiries; being, however, the first from Dr. Hare.

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* See this Journal, 20 : 20–40; 25 : 114–121; 31 : 115–130; 35 : 201–223; also a paper read before the Am. Philos. Soc. 1841, (Trans. N. S. vol. 7,) and copied into the present volume of this Journal, p. 112–119.

Reid on the Law of Storms, Weale, Lond. 1838.

Transactions of the Royal Society of Edinburgh, Vol. 14, p. 467–487.

Poggendorff's Annalen, Jan. 1841, &c.

Piddington's first and second Memoirs on the Law of Storms in India. Calcutta.

† See this Journal, Vol. xli, (July, 1841,) p. 69–77. Do. Jour. Frank. Instit., Vol. 3, third series, p. 40–49.

Moreover, I have sometimes ventured to offer summary sketches of other results or conclusions which seemed to follow from the above mentioned and other developments, which came under notice in pursuing my meteorological inquiries.* These sketches or conclusions were given, partly as *notifications* and partly because I was not willing it should appear in after years, that such results or conclusions as I have noticed had been overlooked in conducting my examinations. These inceptive statements seem to have occasioned many of the "strictures" and criticisms which I am now to notice.

Dr. Hare says that my "idea that tornadoes and hurricanes are all whirlwinds, involves some improbabilities," and that it requires that "during every hurricane, there should be blasts of a like degree of strength coinciding with every tangent which can be applied to a circle," and that "thirty two ships equidistant from the axis of gyration, and from each other, should each have the wind from a different point of the compass with nearly equal force." The only modification he admits, "is that resulting from the progressive motion which tends to accelerate the wind" on one side, "and to retard it upon the other."

I could never have imagined that any "idea" of mine necessarily involved the conditions here specified; and if the fact be such, Dr. Hare would have rendered some service by making it manifest. The modification admitted by him, vitally important as it is, shows only *one* of the conditions which would doubtless prevent any such perfect symmetry of results as he demands; to say nothing of the practical error of supposing that the course of the wind in a whirlwind must coincide with the tangents of a circle. He alleges also, "that as respects any one station, the chances would be extremely unfavorable that the same hurricane should twice proceed from the same quarter." If by this is meant that the changes of wind at any one station in the same gale, are not likely to come back to the same point of the compass from which it had before blown, except by an extraneous force or influence, we shall in this be able to agree. He states further, that "in the course of time it would be felt, at any station, to proceed from many different directions, if not from every point of the com-

* See this Journal, 33: 50–65; also, various incidental remarks and statements in other papers.

pass." The first of these conditions is verified by observation, except as I have shown that the changes in a regular whirlwind storm will not, in the true wind of the gale, be likely to exceed sixteen points of the compass at any one station. It will be difficult, however, for Dr. Hare to show, that the regular changes in a progressive whirlwind storm, as truly exhibited at any fixed station, should run through every point of the compass; although this may sometimes happen to a ship moving in the storm.

Dr Hare does not appear to perceive, that the several conditions above referred to, are for the most part, no more predicable of the whirlwind storm, than of the affluent theory of storms which he advocates.

Dr. Hare states, that "the fact that during the same storm different vessels variously situated, are found to have the wind in as many different directions, may be explained by the afflux of winds from all quarters to a common focal area, as well as by supposing them involved in a great whirlwind." This might be true, as I have virtually stated elsewhere, provided that the direction of the wind at such vessels was found, at a given time, to be towards such a "focal area;" *which does not happen:* the observed differences of the winds from these centripetal directions being nearly equal to ninety degrees, (or a right angle,) as has been repeatedly shown.*

I have formerly stated that "I have observed in the effects of the New Brunswick tornado, numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the vortex at the surface of the ground."† But Dr. Hare thinks, "that the survey of Bache and Espy shows that it would be inconsistent with the facts to suppose such motion, unless as a *contingent* result." Now, without inquiring whether the constant whirling action to which I alluded be a contingent or a necessary result, it is proper to notice, that the great question between us is and has been, *have storms a gyratory character?* To me, the facts established by all the strict observations which have been made and properly stated, proclaim the affirmative. We shall probably find, on a strict examination, that even the

* See this Journal, 25 : 116; 31 : 117-118; 35 : 210-215. Jour. Frank. Instit. 1839, p. 323-336, and p. 363-378. Dové in Poggendorff's Annalen, Jan. 1841. pp. 10, 11, seq.

† See this Journal, 35 : 207.

surveys of Prof. Bache, though not comprising all the particulars which I deem essential to a right view of the case, may yet be best explained by admitting a general and continued whirlwind action.

Dr. Hare next adduces an imperfect quotation on the law of atmospheric circulation, as depending on the earth's rotation, centrifugal action, &c.: and presumes me to mean, "that the centrifugal force communicated to the air at the equator, causes it to rise and give place to those portions of the atmosphere," from adjacent latitudes, which "have less rotary motion;" and proceeds to comment on this presumption. I beg leave to assure Dr. Hare that he has greatly misapprehended my meaning: and furthermore, that I have never found any evidence of the supposed general ascent of the air from the lower to the upper atmosphere in the equatorial regions.

In my first essay, the prevalence of westerly winds in the upper regions of the atmosphere, was incidentally and partially ascribed to the deflection of the trade winds by mountains. Dr. H. alleges that this explanation harmonizes with the theory of Halley. He adds, "In fact as the water accumulated by these winds in the Gulf of Mexico, is productive of the Gulf Stream, is it not reasonable that there should be an aerial accumulation and current, corresponding with that of the aqueous current above mentioned?" This comes nearer to my views of the *course* of circulation in the atmosphere, but does not so well accord with the common theory of the trade winds. That the alleged accumulation of water in the Gulf of Mexico by the trade winds, is the main cause of the Gulf Stream, Dr. Hare may perhaps show hereafter. The contrary would appear to have been settled by the levellings which have already been obtained.

Dr. Hare intimates that the trade winds "cannot be explained without the agency of temperature;" he alleges also that I "reject the influence of heat;" and proceeds to quote a paragraph from which, as well as others, he infers that I "consider gravitation, uninfluenced by heat or electricity, mainly the cause of atmospheric currents;" and he inquires, "what other effect could gravitation have, in the absence of calorific and electrical reaction, unless that of producing a state of inert quiescence?" He also speaks of my treating momentum as "the antagonist of gravitation." [p. 141–142, par. 5–8.]

Now to all this, I answer: 1. That, to my apprehension, the essential features of the trade winds can be best explained without assigning the agency of temperature as the chief moving power. 2. It is an error to say, that I reject the influence of heat. 3. I consider the influences of momentum, centrifugal force, and centripetal action, as being comprised in the laws of gravitation. 4. It is true that I do not consider "electricity" as a general *cause* of atmospheric currents; for the reason, that so far as I know, this has never been shown. 5. That the only effect of gravitation, without calorific or electrical reaction, would be to produce "a state of inert quiescence," in the atmosphere of a moving and rotative planet like our own, is to me inconceivable. 6. I have never considered nor asserted "momentum" to be "the antagonist of gravitation." In the paragraph which is quoted by Dr. Hare, I had suggested *the courses of great storms as indicating the law of circulation in our atmosphere, and which I deemed to be founded mainly on the laws of gravitation.* By some mistake, he has given the phrase "*causes* of great storms" instead of *courses;* and proceeding on this error, he calls it a summing up of the "causes" of atmospheric currents: although he alleges at the same time, that I here admit but *one* cause.

It is next asked, "If the minuteness of the altitude of the atmosphere in comparison with its horizontal extent, be an objection to any available currents being induced by calorific rarefaction," as he states I have alleged, "wherefore should not momentum or any other cause *diminishing or counteracting the influence of gravity,* be on the same account equally inefficient?" To this I answer:—1. Momentum, and the other modifications of the gravitating power, are of far greater magnitude and force than the influence of the mere difference of temperature in the several geographical or climatorial zones. 2. The main tendency or result of this greater force is to produce *horizontal,* not vertical motion. 3. The words which I have italicised, show only the misapprehension corrected above, and which appears to run through the strictures which I am noticing. By "available currents," as above quoted, I here understand the great currents of the atmosphere, constituting the trade winds, &c.

In succeeding paragraphs [10–12] Dr. H. criticises the terms by which I have endeavored to point out, that a whirling or rotative movement *is the only known cause of a violent and destruc-*

tive force in winds or tempests; as the last clause of the paragraph quoted by him should read. There is little probability that my meaning has been misunderstood by general readers; and it appears afterwards to have been divined by Dr. Hare himself.

After a short comment on the functions of gravitation, Dr. H. inquires—"But if neither gravity, nor calorific expansion, nor electricity, be the cause of winds, by what are they produced?" I answer, 1. According to my apprehension, the gravity which induces a nearly equal "distribution of the atmosphere over the surface of the globe," may and does, in its modified influences, constitute the main basis of winds and storms. 2. That calorific expansion is *a* "cause of winds" is universally admitted; but, that it is the *chief* cause I cannot perceive. 3. If "electricity" be the cause of winds, it seems incumbent on Dr. H. to show it.

For my own part, having never attempted to write out or establish a *theory* of the winds, in the common acceptation of the term, nor yet, of the origin or first cause of storms, I have no occasion to go into these inquiries any further than relates to my present purpose. It is true that I entertain some definite views on these points, which have resulted from observation and inquiry; but the choice of time and occasion for their more full development, and also of the evidence on which they rest, belongs to myself rather than to another. I do not intend being diverted from my ordinary business, or from the results of direct observations in storms, by engaging in a controversial discussion of those general views of the alleged cause of winds, and of the physico-mechanics of the atmosphere, which now prevail; and which are held by men of the highest attainments in physical science. And in relation to storms, I have long held the proper inquiry to be, *What are storms?* and not, *How are storms produced?* as has been well expressed by another. It is only when the former of these inquiries is solved, that we can enter advantageously upon the latter.

I have stated, incidentally, that all fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces, &c. Dr. Hare says that, "if this were true, evidently whirlpools or vortices of some kind, ought to be as frequent in the ocean, as agreeably to my observation, they are found to be in the atmosphere." That "the aqueous Gulf Stream, resulting from the impetus of the trade winds, ought to produce

as many vortices in its course as the aerial currents derived from the same source;" and he adds, "there are few vortices or whirlpools in the ocean," for reasons which he has chosen to assign. [14–16.]

Now the alleging that aqueous currents have an equal tendency, with aerial ones, to run into "vortices," belongs to Dr. H., not to me. In the ocean, we can but partially observe the upper surface of superficial currents, moving apparently unobstructed on the more quiescent waters beneath, and with the relative equality of motion in the parts generally maintained. I see not how the unimpeded movements of this denser and nearly non-elastic fluid are to produce vortices equal in number or magnitude to those which occur in the inferior layers of an elastic aerial current, moving on or near the surface of the earth, over obstructions and inequalities, and with other disturbing conditions almost innumerable. Of Dr. Hare's views of aqueous vortices it is unnecessary to speak, but, there are mariners, if I remember their statements aright, who can give him an account of the frequency of ocean or Gulf Stream vortices, somewhat different from that which he advances. Whenever a stream or current of water of moderate depth moves over an unequal bottom, there is found no lack of vortices, of various forms and dimensions, some of which exhibit both upward and downward movements, often of considerable velocity.

Dr. H. doubts if a whirlpool ever takes place without a centripetal force resulting from a *vacuity*. I see not how this doubt can militate against my views of vortical action; but I have myself seen many hundreds of such whirlpools or vortices, and have occasionally watched their developments with much interest.

After commenting on certain arbitrary conditions "of opposing or unequal forces," Dr. Hare desires to be informed how "unequal and opposing forces" are generated in the atmosphere; producing sometimes whirlwinds of unmeasured violence. [17–18.] It may be readily seen, that aerial currents of unequal temperature and velocity, superimposed one upon another, and all moving over a surface of unequal character and with frequent elevations, and subject also to the influence of adjacent currents, must often move *unequally*, and in *unconformable directions;* thus unavoidably running, to some extent, into vortices, eddies or circuits, of various

magnitudes and activity; some of which, may occasionally become extended and spin on an upright and moving axis, with that violent and continued action which characterizes the tornado or water spout. Indeed, it must be obvious, that uniformly direct lines of motion, belong not to our atmosphere or system. But, as before observed, I have here no special concern with the origin of these or other vortices; the simple fact of their existence being all that is necessary for me to maintain.

Dr. Hare then proceeds to state, that in former papers on the causes of tornadoes, he has adduced facts and arguments "tending to prove that the proximate cause of the phenomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created." He also states, that "in this mode of viewing the phenomena, no difference of opinion exists between Espy and himself, however they may differ respecting the cause of the diminution of atmospheric pressure," &c. [19–20.]

I have no desire to offer strictures upon the views of a respected professor of science; but it seems proper here to inquire how an ascending current of air is thus obtained, and whether this *effect*, which perhaps may be due only to an excess of lateral and subjacent pressure, on the exterior of the tornado, be not here adduced as the cause of the effect.

Dr. Hare has been "led to consider gyration as a casual and not an *essential feature*" in tornadoes, and he adduces the dislocation and partial turning of a chimney top on its base, in the New Brunswick tornado, as being due to a *local* whirl within the body of the tornado, and proving that in tornadoes and hurricanes there are local whirls. p. 144. [21.]

I have long since ascertained that local whirlwinds are not of very rare occurrence in great whirlwind storms; the New Brunswick tornado itself having been one of several violent local whirlwinds which occurred within the limits of a somewhat remarkable storm of the above character. This tornado also sent off a duplicate vortex or whirl not long after its passing the Raritan; the path and violent effects of both whirlwinds having been distinctly traced on a field of unripe grain; the smaller one branching off to the right of the main track, where, after causing some prostrations, it passed into the Raritan marshes, and was no more seen. But the whirling motion so far from being only of "casual"

and limited occurrence, appears to be a constant attribute of the tornado; although not always exhibited with uniform intensity and effect in its path, owing apparently, to the frequent rising or narrowing of the vortex, and perhaps other causes.

In his paper as found in the English Journal, Dr Hare says,—"A fact which is admitted by Mr. Redfield, was considered by Espy and Bache, as well as myself, to be irreconcilable with the idea that a general whirling motion is essential to tornadoes. I allude to the circumstance, that when several trees were prostrated one upon the other, the uppermost was found to have fallen with the top directed towards the point towards which the meteor was moving; while the direction in which the lowermost trees were found to have fallen indicated that they were overthrown by a force in a direction precisely the opposite of that which had operated upon those above mentioned."—*Phil. Mag.* [24.]

It is an error to allege that I have "admitted" a fact such as is here stated. On the contrary, in careful explorations made on foot, through an aggregate extent of more than fifty miles of the tracks of various tornadoes, I have never met with such "a fact," or combination of facts, as Dr. Hare describes. In all the cases that I have met with in which trees have fallen one upon another, if their tops pointed in opposite or nearly opposite directions, these directions *have never been parallel to the course pursued by the tornado;* but always in directions more or less transverse to the same: and I consider the opposing allegation as one of the chief errors of my opponents.

The trees which have fallen in directions which are more or less *backward* from the course pursued by the tornado, are almost invariably found *on the left side of the track, exterior to the line of its axis:* But few of these point directly backward, and still fewer can be found near the axis, as the hypothesis of my opponents requires. Of the trees found with their tops pointing *directly forward*, or nearly so, a small number have been seen on or near *the right margin of the track*, with appearances which showed them to lie as they first fell; a fact which seems equally fatal to their hypothesis. Some trees, along and near the line of the axis, are, however, found pointing in this onward direction, and much stress has been laid on *this* fact, by one of my opponents: But it appears, on examination, that in all these cases the trees *have been torn or twisted from the transverse position in*

which they first fell; owing, as I infer, to the more violent force exhibited at and immediately behind the center of the whirl, or at the point which may not inaptly be termed the heel of the vortex.*

It is true, however, as I *have* "admitted," that when trees are found to have fallen one upon another, the top of the uppermost tree points in a direction *more* outward than the one beneath; as is seen by the diagrams and schedules of Prof. Bache, and as may be inferred, perhaps, from the sketches given by Professors Olmsted and Loomis:† And it is *equally* true, that this fact no more favors the hypothesis of a directly inward motion, than that of a whirlwind; but, as an abstract deduction, is "reconcilable" with either. The proper generalization of this class of facts I have attempted to give in my paper on the New Brunswick tornado; which is 'that the uppermost or last fallen of these trees points *most* [or more] nearly to the course pursued by the tornado;' i. e. more nearly than the underlying tree which first fell; divergence from the course of the tornado being still a marked feature of these overlying prostrations.

I have never found a directly backward prostration on the line of the center, or axis, of the tornado. This, as well as the above mentioned facts, will be found sufficiently "irreconcilable" with a direct "afflux of the wind from all points of the compass," 'in a central and non-whirling course,' "towards a common focal area."

In the same Journal, Dr. Hare says he "cannot understand how the opposite forces belonging respectively to the different sides of the whirlwind, can be made to bear successively *upon one spot*, so as to cause trees to fall in diametrically opposite directions." *Phil. Mag.* [25].—Neither can I understand this, if each of these "opposite directions" *be parallel to the course of the tornado*, as is alleged by Dr. Hare, in the passage last noticed.

Dr. Hare next tells us—"A fact, irreconcilable with a general whirling motion, has been recorded by Messrs. Espy and Bache. A frame building was so situated as to be protected by another edifice in one direction from the suction of the tornado, and yet was exposed to its influence as it advanced, and as it moved away. Hence two of the four parts, on which the frame

* See this Journal, July, 1841, pp. 69–79.

† See this Journal, Vol. xxxiii, p. 369; Vol. xxxvii, p. 343.

rested, were so impelled by the wind as to make furrows in the ground, of which one was nearly at right angles to the other. Evidently such furrows could not rise from the transient tangential impulse of a whirlwind." pp. 144, 145. [22.]

In the English Journal, Dr. Hare alleges that one of the four posts on which the building was supported, "*was first moved towards the tornado*, as it advanced:" while Prof. Bache shows us that the tornado advanced from south 80° west, to north 80° east; and that the posts were first moved "*to the west of north.*"

But on what grounds this "fact" is pronounced "irreconcilable with a general whirling motion," I am wholly unable to perceive. For, had he closely examined the whole case, he would hardly have failed to see that the movements of this building, as described by Prof. Bache, are fully "reconcilable" to an involute "whirling motion," such as I allege to be characteristic of these tornadoes; and that there was no necessity for resorting to the gratuitous hypothesis of its being "protected by another edifice in one direction," or even that of "the suction of the tornado."

If a whirlwind figure having a diameter of three or four hundred yards by the scale of Prof. Bache's figure, [Plate III, fig. 3,]* be drawn on tracing paper, with involute whirling lines representing, horizontally, the course of the wind from the exterior to the interior of the tornado, and if the center or axis of this figure be passed from west to east along the line pursued by the axis of the tornado as indicated on the plate, revolving at the same time to the left with a velocity greatly exceeding its advancing motion, it may be seen that the wind of the whirl will be indicated as beginning at this building from nearly south, *i. e.* moving "to the west of north," nearly, or in the general direction of the first furrows in the ground. It will also be seen, that the wind of the whirl, changing by southwest, and having its gyrations quickened near the center, would, immediately after the passing of its axis, exhibit its greatest force from the western quarter, corresponding to the second movement of the posts in the ground; the wind veering from thence towards the northwest as the tornado passed away: thus showing two directions of wind which sufficiently coincide with the first movements of the posts of the building "to the west of north," and subsequently "to the

* See Jour. of Franklin Institute, Vol. III, third series, 1841, pp. 273 and 276.

eastward," or "nearly at right angles" to its first course; according to the descriptions and plan of Prof. Bache, who gives the course of the axis as "east 10° N.," the building being to the southward or on the right of this line.

I say nothing here of the protection afforded by "an edifice" which after the first moment, according to the hypothesis of motion adopted by Messrs. Espy and Hare, was constantly more or less to *leeward* of the building so protected. By applying to Prof. B.'s plan, as before, a compass card, moved from west to east without revolving, we shall find their wind to commence nearly at east, passing thence through south to southwest, and possibly to west southwest, near which it would terminate. Thus, the first effects of the wind, when, even upon the hypothesis of "suction," the building was unprotected, could not produce the first motion in the direction "to the west of north," which may perhaps be fairly taken at 5° or 10° west of north; and the wind, on their hypothesis, would hardly appear to have reached a point which could produce the second movement "to the east."

I have been thus particular in this examination, because the case thus alleged by Dr. Hare is a further specimen of the erroneous inductions which have been made and relied on by my opponents. In examining the plans referred to, it should be observed, that the sketch of prostrations in the orchard, which is included in fig. 3, is evidently on a more reduced scale than that given in the plan of the building; otherwise, the buildings must be of size sufficient nearly to have covered the orchard. This change of scale may cause some confusion unless particularly noticed.

That the velocity and consequent force of the whirling movement of the tornado is maintained by the direct *pressure* of the surrounding atmosphere, rather than by the "suction" alleged by Dr. H. I can readily conceive; but that the "impulse of a whirlwind" of this character is generally found to be "tangential" to its axis, which he seems to consider a necessary condition, I do not admit.

Dr. Hare appears to concede, that my survey of this tornado shows effects which accord with whirlwind action; but he seems desirous of limiting this admission to the prostration of "certain trees," and alleges that this survey "does not demonstrate gyration to be an essential feature of tornadoes," and that "it is sufficiently accounted for by considering it as a fortuitous consequence

of the conflux of currents rushing into a space partially exhausted." [23.]

Now I cannot but think, that readers who have no theory to support, will view the results of my survey in a very different light. Dr. Hare omits to mention, that the survey comprised the entire breadth of the visible track, at perhaps its broadest place; that it was intended to include every tree prostrated within its limits; that it essentially agrees with the main features of the more partial surveys of Prof. Bache; that I have shown by clear inductions from all the prostrations in the survey that the whirling motion was one general effect, comprising the entire width of the track; that the tornado must have arrived at this ground in nearly its most perfect action, having just left the surface of the Raritan river; that the axis of prostration was not found in the center of the track, but nearest its left margin; that the main rotation was wholly to the left or in one constant direction; and, that the leading features of the prostration found in this survey, have also been observed as constantly occurring, in the tracks of many other tornadoes.*

I may add, that in a careful exploration of the track of this tornado for several miles, I found nothing to contravene the results presented in my published survey; the general features of the prostration being greatly analogous to those which I have given.

Dr. Hare thinks it singular, that I should have declined noticing the "insuperable difficulties" of the hypothesis of 'a central and non-whirling course in the wind of the tornado,' to which I have alluded in bringing forward facts and inductions which seem to contravene this hypothesis. He states, also, that "the advocates of the disputed hypothesis are not aware of any such difficulties," and intimates the impropriety of the allusion "without naming the facts and arguments" which justify it. [24.]

I considered it more proper, however, to rely solely on the survey and inductions which I then presented; as these appear sufficient to set aside, not only the hypothesis itself, but also some of the chief deductions from the phenomena of this tornado which

* See this Journal, 41: 69–77. Do. Jour. Frank. Instit. Vol. 2, third series, p. 40–49.

have been put forth and relied on by Mr. Espy and Dr. Hare.* Besides, I had no wish to assume a controversial attitude, in assailing by argument, an hypothesis which virtually discards the observations of mankind in all past ages down to the year 1835. The testimonials of these observations appear in the names and terms applied by all people in all languages to this small but violent class of storms. "The facts" demanded, I had supposed, were furnished on that occasion in sufficient numbers.

Dr. Hare next adduces "the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water which had risen up, as if boiling, within a circle of about three hundred feet, subsided as often as a flash of lightning took place;" which he alleges to be a "fact which is utterly irreconcilable with Mr. Redfield's "rotary theory." He adds: "now supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electric discharge cause it to subside?" [25.]

For the supposition here made, as well as for "the water which had risen up," Dr. H. seems alone accountable; as his witness, Mr. Allen, speaks only of "the effervescence produced by the tornado in the water" having "perceptibly abated." The water he states to have been "in commotion like that in a huge boiling caldron;" but, *that which rose up* from the surface, he describes as "misty vapors resembling steam," which "after the flash, seemed sensibly to diminish for a moment."† I cannot perceive that the fact thus alleged has the least unfavorable bearing upon my views of rotative action. Therefore, without considering the optical effect which may result from a flash of lightning, or the immediate conversion of clouded vapor into rain, which oftentimes suddenly follows, I will only state, that another competent observer, who was very near this whirlwind when it left the western shore and who watched its progress across the river, has described to me the appearance of the cloudy sprays or mists blown from the surface of the water, and which filled the lower extremity of the tornado, but he has mentioned no sudden disappearances of the same. He did, however, observe the *whirling action* of the tornado with great distinctness, both when it

* See Journal of the Franklin Institute, Vol. 20, new series, 1837, p. 56–61; also Vol. 2, third series, 1841, p. 356–359.

† See this Journal, Vol. xxxviii, p. 76.

first entered upon the river, and in its effects upon the sails and position of a schooner with which it came in contact; and likewise, as exhibited by the circling or whirling directions of the various objects carried into the air, as it came off the high grounds on its approach to the river. The highly intelligent eye-witness of my opponent, also describes "the misty vapors" as "*entering the* WHIRLING VORTEX;" thus showing from his own observation, a fact which fully supports my views, and is fatal to the objections, and hypothesis of motion, set forth by Dr. Hare. Moreover, there were decisive memorials of a general whirling action found along the path of this tornado.

Dr. Hare chooses also to say, "that the explanation which Mr. Redfield dignifies with the title of his 'theory of rotary storms,' amounts to nothing more than this, that certain imaginary nondescript unequal and opposing forces produce atmospheric gyration, that these gyrations by their consequent centrifugal force, create about the axis of motion a deficit of pressure, and hence the awful and destructive violence displayed by tornadoes and hurricanes."—"I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces, on which it is built, exist only in the imagination of an author who disclaims the agency either of heat or electricity." p. 145. [26–27.]

The recital of this passage appears necessary on account of the gross error into which Dr. H. has here fallen. I have never attempted to dignify any "explanation," induction, sketch, or essay, "with the title" of my "theory of rotary storms." It must, at least, have been a mistake of person. I have little fondness for theory-making; and as little respect for hypotheses of winds or storms, other than those which result directly from sufficient and reliable observations. Neither have I disclaimed "the agency of heat," as already stated; but it may have been my offense to have disclaimed "electricity" as a known cause of storms. My cursory explanations of the action of a whirlwind or tornado, even as shown up by Dr. Hare, are, in my view, better suited to the observed facts of the case than any which he or Mr. Espy has offered.

I do not solicit for my views even that "smallest importance" which is denied them in the mind of my critic; but the attention with which he has treated them, both here and abroad, does not appear to agree well with the disavowment. With the facts

before him which are shown in my survey of the tornado, and also with the numerous observations made in great storms, which I have published, it is both vain and absurd to pretend that my views of their rotation are founded only in imagination. I am not conscious of having "built" or indicated any "theory," views, suggestions or explanations of storms or whirlwinds which have not been based on observations of my own and facts otherwise ascertained, sufficient in my view to warrant them; the 'unequal and opposing forces' even included: although, I have not always urged these facts upon the attention of my readers; having, not unfrequently, reserved them for more appropriate occasions. Hence, my alleged proofs have been chiefly confined to the progressive course and rotative action developed in storms; which last, strangely enough, has been so pertinaciously denied by Mr. Espy, and now by Dr. Hare.

My opponent next attempts to show, "that any deficit of pressure about the axis" of a whirlwind, "consequent to the resulting centrifugal force, could only cause in the atmosphere a descending current, while it could not tend in the slightest degree to carry solids or liquids aloft." p. 146. I was also surprised to find this hypothetical downward current in the midst of a whirlwind alleged as a necessary condition, on former occasions, by Mr. Espy. If the allegation be true, it must be easy to show that the ascending currents in chimneys should become inverted: for, so far as simple gravitation is concerned, it can make little difference whether the rarefaction be mechanical or calorific.

But the ascending effects in the interior of a whirlwind have been too often witnessed by myself and others to require discussion. Indeed, it would almost seem that the objectors had been precluded from all opportunities for correct observation. There are numerous cases, however, in which the upward movement of the objects elevated cannot be seen in the central and lower parts of the whirlwind; owing, as I have had good occasion to know, to the great angular velocity of the central gyrations.

Dr. Hare appears to suppose, that gyration in a revolving fluid mass will not quicken as it approaches the center, unless as resulting from a centripetal force "caused by suction at the axis."

A constant centripetal *force* I have already recognized on this as well as former occasions. But this by no means requires or produces a direct centripetal *course* in the moving air which

yields to its influence. But in the cause assigned for this force, as well as in the specific directions of the movements produced, we differ essentially. So far from ascribing this quickened gyration to the "suction" alleged by Dr. Hare, I know of no such power in the uninclosed atmosphere; conceiving, that neither rarefaction nor any other known cause can here occasion "suction," according to the common use of this term. Air, whether rarefied or not, can never ascend but in obedience to a *pressure* or *force*, sufficient to exceed both its own weight and that of all the atmosphere which lies immediately above it, or in the immediate direction or locality of its motion. This erroneous hypothesis of "suction," in some form or other, appears to lie at the bottom of the various speculations and inductions of my opponents.

In noticing the spirally involute and quickening motion which I allege as observable in 'all narrow and violent vortices,' Dr. H. gives an erroneous reference for his quotation; and the latter seems also to be somewhat inaccurate. I do not see that his speculations on this quickened motion 'towards the center or axis of the whirl,' can affect either my views, or the disputed fact of gyration; and they are sufficiently answered by observations published in my first paper,* as well as by the remarks made above on centripetal force.

Dr. Hare thinks that so far as my observations show the quickening of the whirling motion towards the center of the tornado, they tend to confirm the views of my opponents and to refute those which I uphold. To me it appears that this is an entire abandonment of his ground. It is the general fact of gyration which I am chiefly concerned to uphold, and which has been combated by him and his predecessor in this controversy. I dispute with no one as to how it may be produced. Should better explanations of this fact than mine be offered, they will be cheerfully adopted. In the mean time, I shall adhere to my observations and opinions, rather than to the hypotheses and speculations of my opponents.

Dr. Hare thinks, "that any theory of storms which overlooks the part performed by electricity, must be extremely defective." I do not perceive that the part performed by electricity in a gale

* See this Journal, Vol. xx, p. 45–46.

of wind, squall, tornado, or other storm, ever constitutes an essential feature of the same: but, the part so performed, appears to me to be only incidental and subordinate to the action and main effects of the storm. Electricity is not wind, nor water, nor vapor; but an imponderable matter or effect, which is not known to exert any constant mechanical force or action upon the effective currents of the atmosphere. "Thunder and lightning and convective discharge," are but momentary or transient exhibitions of electricity, producing no visible effects upon these currents; whatever may be their agency in restoring the disturbed equilibrium of the different atmospheric elements. The electricity developed by a steam boiler is not considered as producing the steam or its jet, or the condensation of the latter; but is itself produced by these. Even were it shown that a stream of electricity was constantly developed between the rarefied column of a moving tornado and the surface beneath, I cannot see how this could be assumed as the *cause* rather than the *effect* of the local rarefaction. If the part which electricity performs in a storm be essential, or controlling, its functions ought to be distinctly pointed out.

I would humbly suggest that the old practice of forming or inventing theories or schemes of action for the powers of nature, ought to be mainly abandoned. The Wernerian and Huttonian theories are well remembered; and how small would have been the progress of the science to which they relate, had its cultivators continued to exhibit only the spirit and philosophy of the early advocates of these theories; and how much less, if guided by a philosophy so speculative and untenable as that of the affluent and up-moving hypotheses of winds and storms? More strict and extended observations and inquiry, with greater caution in the adoption of hypotheses, whether old or new, would in my opinion, tend greatly to the advance of meteorological science.

Observation, rather than "lucubration," has been my employment when exempted from other duties: and if the results of observation do not accord with the "lucubrations" of Mr. Espy and Dr. Hare, I conceive that I am in no degree responsible for the difficulties of their position.

New York, January 13, 1842.

ART. XIII.—*Abstract of the Proceedings of the Eleventh Meeting of the British Association for the Advancement of Science, held at Plymouth, September*, 1841. [*Prepared from the Report in the London Athenæum.*] Concluded from page 164.

Sect. B. *Chemistry and Mineralogy.*

Mr. R. HUNT communicated a paper on the *influence of the ferro-cyanate of potash on the iodide of silver, producing a highly sensitive photographic preparation.* The author being engaged in experiments on that variety of photographic drawing which is formed by the action of the hydriodic salts on the darkened chloride of silver, with a view to the removal of the iodide formed by the process, from the paper, was led to observe some peculiar changes produced by the combined influences of light and the ferro-cyanate of potash. He found that the ordinary photographic paper, if allowed to darken in sunshine, and then slightly acted on by any hydriodic salt, and washed when dry, with a solution of the ferro-cyanate of potash, became extremely sensitive to light, changing from a light brown to a full black, by a moment's exposure to sunshine. Following out this result, it was discovered that perfectly pure iodide of silver was acted on with even greater rapidity, and thus it became easy to form an exquisitely sensitive photographic paper. The method recommended is the following: highly glazed letter paper is washed over with a solution of one drachm of nitrate of silver to an ounce of distilled water; it is quickly dried and a second time washed with the same solution. It is then, when dry, placed for a minute in a solution of one drachm of the hydriodate of potash in six ounces of water; and being placed on a smooth board, gently washed by allowing pure water to flow over it, and dried in the dark at common temperatures. Papers thus prepared may be kept for any length of time, and are at any moment rendered far more sensitive than any known photographic preparation, except the Calotype, which it quite equals, by simply washing it over with a solution formed of one drachm of the ferro-cyanate of potash to an ounce of water. These papers may be washed with the ferro-cyanate and dried in the dark: in this dry state they are absolutely insensible, but they may at any moment be rendered sensitive by merely washing them with a little cold water. The

paper is rendered quite insensible by being washed over with the above hydriodic solution, and from the photograph thus fixed many copies may be taken.

Some researches on the development of the Electrical Force, and an inquiry into the nature and properties of the New Element or product of electrical action described by Schönbein, by Mr. F. De Moleyns. The statements made by Prof. Schönbein at the Glasgow meeting (see Vol. XLI, p. 43) respecting the new element which he called *ozone*, attracted the attention of Mr. De Moleyns; and the paper now read contained some of the more important results of his experiments. In the report alluded to, Prof. S. states that the disengagement of the "odorous substance" depended, 1, upon the nature of the positive electrodes; 2, upon the chemical constitution of the electrolytic fluid; and 3, upon the temperature of that fluid. He added, that his experiments went to show that well-cleaned gold and platina were alone capable of disengaging the odoriferous principle, and that the *more easily* oxidable metals, as well as charcoal, did not possess that property at all. The results of Mr. De Moleyns's investigations appear to prove: 1, that the disengagement of the peculiar odor is not confined to the *less easily* oxidable metals: 2, that by certain arrangements, *all metals*, when positive electrodes, may be made to develope the odoriferous principle: 3, that certain positive metals, not acting as electrodes, will evolve this principle: 4, that charcoal forms no exception to this rule: 5, that all substances, whether crystalline in structure or otherwise, possessing the property of appearing luminous by friction, or of yielding sparks when struck, also possess the property of discharging, under such circumstances, the peculiar odor: 6, that iron and nickel develope this principle more strongly than any other metal. Mr. De Moleyns, observing the odor to be produced at the points connecting an electro-magnetic machine with the battery, constructed an apparatus by which magnets were made to revolve within a glass cylinder, which could be exhausted at pleasure, or filled with various gases; by such means he obtained a vacuum, and operated in dry air, collecting the matters evolved over distilled water, and by such modes he proved that ozone could not only be evolved in a dry atmosphere, but also in a vacuum—mercurial and common. These and other experiments led Mr. De Moleyns to the conclusion that the *ozone* of Schönbein, which he proposed

to name *electrogen*, must be admitted into the list of supposed elements: that it was *not* a union of an electrolytic compound whose action was *unknown;* and that probably it exists in combination in various forms of matter, which at present are considered elementary, but which in reality are not so.

A paper on *manures considered as stimulants to vegetation*, was communicated by Dr. Daubeny. The author discusses the question as to the sense in which manures can be considered to act as stimulants to plants. It is evident that if the term *stimulus* be understood in an acceptation similar to that in which it is employed with reference to the animal economy, it ought to be confined to bodies, which by their presence, assist in promoting the secretion and assimilation of the nutritious materials present, and ought not to include such as themselves afford materials for secretion. Thus salt and other condiments do not themselves nourish the animal; but by their presence, induce its secreting surfaces to assimilate more readily the substances presented to them. Now, it becomes a fit subject for inquiry, whether manures operate in the former manner or in the latter; and likewise whether the fact, that certain of them act less beneficially at subsequent periods of their application than they did at first, admits of being explained on the recognized principle that "stimuli lose their full effect upon living matter when frequently repeated." Dr. D. adduced several facts which led to the inference that the nitrates of soda and of potassa operate favorably upon certain crops by communicating to them nitrogen; and the reason why these salts sometimes have appeared to leave the land in a worse condition than before their use, is not owing to their being stimuli, and therefore amenable to the law above quoted; but is because the free supply of nitrogen afforded by the decomposition of the nitrates, had caused the plant to absorb a larger portion of those other ingredients, such as phosphate of lime, silicate of potassa, &c., which are present only in a limited quantity in the soil, thus tending to exhaust it of these materials, and causing thereby an inferior crop to be produced on the following year. Now, though it may be true that the nitrates in this manner indirectly stimulate the vital energies of the plant, yet it was conceived that the term *stimulus* had better be abandoned with reference to such cases, as its adoption might lead to an erroneous impression in the mind of the farmer with respect to the proper

mode of restoring to the land its original fertility. If the theory suggested by the author be the true one, it will follow, that the proper remedy would be, not to discontinue the use of the nitrates, but by the application of bone manure, &c. at intermediate periods, to restore to the land those other ingredients which had been abstracted from it in too large a quantity. To determine what materials are wanting, and in what proportions they ought to be applied; (independently of the empirical plan of ascertaining, by repeated trials, the substances, which, by their addition succeed best in remedying the deficiency,) two methods present themselves. The first, a difficult one, is to learn by a minute analysis of the soil, whether the ingredients which the crop requires are actually present, and to add of these a quantity equal to that which the intended crop is calculated to contain. The second, a more practical scheme, is to estimate in the first place, how much of these substances exists in the crop taken off the ground, and then to add to it at least an equivalent quantity of manure. The Doctor suggested, that in farming establishments, a kind of book-keeping should be undertaken, on this principle: a debtor and creditor account being made out of the quantity of nitrogen, of earthy phosphates, of alkali, &c. abstracted in the form of crop, and restored in that of manure each year. He concluded by specifying certain points relative to this subject which require further investigation. 1. To confirm or disprove his theory, with respect to the operation of the nitrates, by determining whether they actually diminish in quantity, and finally disappear after several successive crops have been grown upon land impregnated with these salts. 2. Whether the same applies to common salt and other mineral manures as to the nitrates, or whether any of them act directly as stimuli. 3. More extended and exact data relative to the amount of alkaline and earthy salts, and of nitrogen present in the various crops cultivated by the farmer, as well as in the manures he employs.

A practical method of determining the quantity of real Indigo in the Indigo of Commerce, by Dr. Samuel L. Dana, of Lowell, Mass. U. S. A. Dr. D. directs that 10 grains of indigo reduced to an impalpable powder should be boiled in a Florence flask a few minutes, in 2½ oz. of a solution of carbonate of soda, making 30° to 35° on Twaddel's hydrometer; then add 8 grains of crystals of muriate of tin, and boil for half an hour: a beautiful

yellow solution of indigo will be obtained. Withdraw the flask from the lamp, and introduce into the solution 500 water-grain measures of a solution of 50 grains of bichromate of potash in 4000 grains of water. The indigo blue, with a trace of indigo red, will be precipitated, while the other components remain in solution. Filter the precipitate through a double weighed filter, washing the mass with 1 oz. of muriatic acid, diluted with 3 oz. of boiling water; wash with hot water till water only returns; separate, dry and weigh the filters; note the weight of the precipitate, burn one filter against the other; the difference is the silica contained in the indigo, which deducted from the weight of the precipitate, gives the quantity of pure indigo. Mr. Walter Crum, who communicates the above, adds, that carbonate of soda with protoxide of tin does dissolve indigo, and forms a yellow solution, but so slowly that he doubts if all the ten grains are acted upon. He thinks Dr. Dana must mean soda-ash, which contains a notable quantity of caustic soda; but a much weaker solution of caustic soda would answer the purpose.

On the disintegration of the Dolomitic Rocks of the Tyrol, by Prof. Daubeny. The author attempted to explain, without resorting to volcanic agency, the abrupt form, extraordinary height, naked outline, and fissured surface of the dolomitic rocks of the Tyrol. He attributed these characteristics to the slow rate at which decomposition proceeds in rocks consisting of pure dolomite, and the strength of the cohesion which binds together the particles of this rock, owing to which, even those portions which stand prominent in consequence of the removal, by the agents of destruction, of their contiguous parts, often remain unaffected by those mechanical forces which would cause the projecting portions of a rock less unyielding in its texture, to become detached. The cause therefore of the greater height maintained by the dolomites of the Tyrol, than by the pyroxenic rocks which accompany them, seems to be the inferior rate at which decomposition has proceeded in the former, whilst the bold and jagged outline they display, may have been produced by the tenacity with which their parts cohere. The sterile character of these same rocks, even in parts which are not precipitous, appears to be owing to the slowness with which they decompose, as well as perhaps to the absence of organic remains. The Professor concluded with some suggestions as to the means of fertilizing rocks containing

magnesia, when from the slowness of their decomposition they continue sterile; and proposed in such cases to accelerate the disintegration by pouring upon the sub-soil dilute sulphuric acid.

Mr. Prideaux communicated the *results of inquiries into the causes of the increased destructibility of modern copper sheathing.* Experiments made on various kinds of copper sheathing immersed in sea water, showed, that in the laboratory, under parallel circumstances, they do not observe the same order of durability and waste, as they had done in use. The cause of comparative waste appears therefore to be, in part at least, due to *external conditions;* and of these, two classes may be noticed; one depending on the connexion with the ship, the other on the circumstances of her employment. Of the first class, two suggested themselves,—the position on the ship's side, and the nails by which the copper is fastened. The lower part of a ship's copper seems to suffer much less than the upper, so long as she continues in deep water; but when she grounds at low water, if on black mud, this part suffers most from the action of sulphuretted hydrogen, peeling off in blue flakes. The influence of the nails offers rather more chemical interest. They are never of pure copper, and being very numerous, all in contact with the copper sheets, whilst their heads present also a considerable metallic surface to the salt water, they may produce very decided effects, either preservative or destructive, by a slight electro-chemical difference. Various experiments were tried, which proved that most nails are destructive. The conclusion resulting was this, that the nails might be rendered slightly electro-positive to rolled copper, by the addition of zinc, which would not injure their flexibility nor enhance their cost. The test by the galvanometer would, after a little practice, be easily applied, in making up the metal for casting them.

Another mode of protection is offered, by coating the copper when new, with fish oil, which in one instance has been of signal service. The preservative effect of coal-tar was also noticed. This tar had trickled down over the copper from the wood-work above, and had crossed the sheets just where most subject to the wash and friction; and whilst the naked metal had been quite worn away, the coal-tarred streaks remained entire; the surface of the copper, on melting off the tar, being as perfect as when fresh from the roll. But it remains to be seen whether it will keep a clean surface free from organic adhesions and earthy incrus-

tations. Experiments are now in progress to determine this question.

New extemporaneous process for the production of Hydrocyanic Acid for medical use, by R. D. Thomson, M. D. The importance of this acid as a remedial agent, induced the author to bestow much attention upon a mode of producing it always of uniform strength. Having tried all the processes for this purpose, proposed in this country, he was satisfied that none of them afforded an acid of uniform strength. The process recommended by Dr. Clark of Aberdeen, was superior to every other, but an objection to it is the great difficulty of procuring pure cyanide of potassium. The author believes the following process to be less liable to objection than any at present used. The first step consists in forming a pure cyanide of lead. This may be done in various ways, either by precipitating acetate of lead by hydrocyanic acid, as prepared from the ferrocyanide of potassium and sulphuric acid, in a stoppered bottle, or by distilling the mixed materials into a Wolff's bottle containing a solution of acetate of lead. In either case a definite compound of cyanogen and lead will be obtained, which is to be carefully washed and gently heated. The next step in the process is to decompose it by means of sulphuric acid. In order to obtain an acid of the strength of the *Acidum Hydrocyanicum dilutum* of the London Pharmacopœia, or containing about two per cent. of absolute acid, the following formula is recommended.

46.36 grains of cyanide of lead.
2 fluid drachms of dilute sulphuric acid. Lond. Pharm.
6 fluid drachms of pure distilled water.

Introduce the cyanide of lead into a stoppered bottle; mix the acid and water in a glass vessel; allow the mixture to cool, and then pour upon it the cyanide of lead. Close the stopper, and agitate the fluid and salt together. After standing for some time, pour off the supernatant liquor from the precipitated sulphate of lead, and preserve it in a stoppered bottle. This formula is founded upon the circumstance that the dilute sulphuric acid of the London Pharmacopœia contains in each fluid drachm about 9.5 grains of oil of vitriol (SO^3HO). Two drachms will therefore contain nineteen grains of oil of vitriol. The quantity required for saturating 43.36 grains of cyanide of lead, is only 17.4 grains; but the small excess is useful in preserving the acid.

On the radical of the Kakodyle Series, by Prof. Bunsen.—The easiest method of procuring pure kakodyle is the following. Chloride of kakodyle, carefully freed from the oxide by treatment with strong hydrochloric acid, is allowed to stand some time over chloride of calcium and quick lime, to remove the water and all excess of acid. It is then introduced into a distillatory apparatus carefully filled with carbonic acid, and containing some slips of clean sheet zinc. Any of the metals which decompose water will answer, but zinc is the best. It is probable that hydrogen or carbon would produce a similar decomposition with suitable modifications of the apparatus. The vessel is then hermetically sealed, and the mixture of zinc with the chloride, is exposed for some hours in a water bath to a temperature of 212° F. When the decomposition is complete, a white saline mass is formed, which melts into an oily liquid between 240° and 248° F.; while the tube is still hot, the point of the tube leading into the condenser is dipped below the surface of boiled distilled water: as the apparatus cools, the water rises into it. The tube is hermetically sealed: the water dissolves chloride of lime, leaving the excess of zinc and the *kakodyle*, which falls as an oily liquid to the bottom. This is rectified twice or three times, filled with carbonic acid as before; the water being afterwards removed by chloride of calcium in the usual way. Thus obtained, it is a colorless liquid, transparent and of a high refractive power, in appearance and odor much resembling the oxide of kakodyle, and ignites instantly on being brought in contact with air, giving off water, and carbonic and arsenious acids.

Abstract of a letter from Prof. Liebig to Dr. Playfair.—This letter announces the discovery of a white crystalline substance, in large quantities, obtained by M. Schunk from the lichens which are employed in preparing archil, (*Lecanora tartarea*, &c.) by extraction with ether. It differs from *erythrine*, and the compounds described by Dr. Kane, in its insolubility in water. It dissolves readily in alkaline solutions, and is capable of being again precipitated by acids, if the solution be recently made; but if kept standing for some hours, acids produce no precipitates: it has been decomposed, and is converted into carbonic acid, and *orceine*. If the substance be dissolved in baryta water, and the clear solution boiled, a large precipitate of carbonate of baryta occurs, and the filtered solution gives, on evaporation, large quan-

tities of *orceine*. From this circumstance a number of phenomena in the color of lichens can be explained, which Dr. K. has described in his work on that subject. Prof. L. also states that he has performed many experiments on the legumin of beans, and some other leguminous plants. He has arrived at the conclusion, that this body is identical with the casein in milk of animals. It has precisely the same composition, and contains the same salts,—(phosphate of potash, potash, magnesia, lime and iron,)—as the casein of milk. Prof. L. also mentions, that Drs. Will and Varrentrapp have devised an excellent method for determining the amount of nitrogen in organic bodies. The substance is mixed with a quantity of caustic potash and hydrate of soda, and heated to redness in an ordinary combustion tube. All the nitrogen in the substance escapes as pure ammonia, which is condensed in a small and neat apparatus, containing dilute hydrochloric acid. This solution is mixed with chloride of platinum, evaporated to dryness in a water-bath, and the excess of chloride of platinum is washed from the ammonia chloride by a mixture of ether and alcohol. From the metallic platinum which remains after the ammonia chloride is heated to redness, the quantity of nitrogen is to be calculated. In conclusion, the Professor states that he has repeated all the experiments of Dr. Brown on the *production of silicon from paracyanogen*, but is not able to confirm one of his results. His experiments prove that paracyanogen is decomposed by a strong heat, into nitrogen gas, and a residue of charcoal which is exceedingly difficult of combustion.—Dr. Parnell stated that he too had repeated the experiments of Dr. Brown, without being enabled to verify any one of his results.

The following papers were also read before the Section.

On some instances of restrained chemical action; by E. A. Parnell.

On some subjects connected with the sulpho-cyanides; by the same.

On the direct formation of cyanogen, from its elements; by G. Fownes.

Experiments showing the possibility of fire from the use of hot water in warming buildings, and of explosions in steam-engine boilers; by Goldsworthy Gurney.

On the production of sulphuretted hydrogen by the action of vegetable matter on solutions containing sulphates; by E. Lankester, M. D.

On the composition of crystallized diabetic sugar; by R. D. Thomson, M. D.

On spontaneous combustion; by Messrs. Booth, Hunt, and Hearder.

Section C. *Geology and Physical Geography.*

Mr. J. E. Bowman read an extensive paper *on the Upper Silurian Rocks of Denbighshire*, and stated that a re-examination

had furnished him additional proof of the correctness of the arrangement of these rocks, which he had proposed in a paper read at the Glasgow meeting.

A report was read from the committee appointed at Glasgow, for obtaining instruments and registers to *record shocks of earthquakes in Scotland and Ireland.* Two instruments were devised and set up for this purpose, about January 1, 1841, near Comrie, in Scotland, viz. the *common Pendulum Seismometer*, and the *inverted Pendulum Seismometer.* No very important results had hitherto been obtained, and it is now proposed to adopt new instruments of greater scope and sensitiveness than those before employed.

On the *occurrence of some minute Fossil Crustaceans in Palæozoic Rocks*, by Mr. John Phillips.—After mentioning various places in which these animals have been found, he states that he had lately observed in Pembrokeshire, in the lowest shales of the mountain limestone, within ten feet of the old red sandstone, beds of Cyprides very similar to those in the black shales of the upper coal measures in Manchester. These are probably the most ancient specimens of the group yet discovered. The circumstances under which these Crustaceans are found at the present day, appear to agree with those attending their occurrence in a fossil state; the recent Cyprides seem destined to consume the perishing parts of animal and vegetable substances, and the fossil species are generally associated with portions of fishes near Manchester, and elsewhere. Probably these remains occur under many circumstances, but to ascertain all the conditions under which they lived, requires attention to many sorts of strata not often suspected to contain remains. Very remarkable conditions occurred when the old red sandstone ceased to be deposited: for then, after a long series of formations, with no trace of organic remains, we find in the beds immediately above, thousands of minute Crustaceans, bone beds, layers of Brachiopoda, &c. of marine origin; and encouraged by this example, we may expect to find them in beds of still higher antiquity.

A paper was read by Mr. W. Walker, on the *Geological Changes produced by the* Saxicava rugosa *in Plymouth Sound.*—The *S. rugosa* appears to be the prevailing perforator of the limestone rocks, and it is the author's opinion that these operations have been carried on during such long periods as to destroy rocks, and make deep water where shoals previously existed.

An account of the Fossil Organic remains of the southeast coast of Cornwall and of Bodmin and Menheniott, by C. W. Peach.—The line of coast examined commences at Veryan, four miles south of Tregoney, and extends eastward by Gorran, the Blackhead, and Fowey, to East Looe. The cliffs are composed throughout of quartzose and slaty rocks, hitherto supposed by Mr. Conybeare and others to be destitute of fossils. But along the whole line, Mr. P. has detected traces of Brachiopodous shells and corals, and the stems of encrinites are of frequent occurrence. From Veryan to Gorran the quartzose rocks rarely contain traces of shells, but in the calcareous slates in contact with dykes of greenstone at Blackhead, remains of corals resembling Turbinolepsis, and of the genera Cyathocrinus, Spirifera, and Orthoceras occur. Eastward, at Pridmouth, a fine specimen of the Platycrinite was found, with the column, pelvis, arms, &c. In the slate quarries of Fowey, remains supposed to be those of fish, and corals of the genus Favosites were detected. Near Polman, occur encrinital stems nearly a foot long, together with remains of trilobites, corals of the genera Cyathophyllum, and Favosites, Spirifers, Orthoceras, and a fossil with a structure resembling that of the Sepiadæ. At Pentlooe, an equal-valved bivalve, resembling Uncula, and a species of Orthis, have been found; and at East Looe another fine specimen of an encrinite, with column, arms and tentacula attached; also specimens of Cyathocrinus, Fenestella, Turbinolopsis and Orthis. At Bodmin, the author has detected encrinites in the slate quarries, and in those of Menheniott in Liskeard the eye of a trilobite in good preservation. On the beach below the cliffs at Port Mellin, near Mevagissey, the author observed traces of a lacustrine deposit, containing roots and branches of trees, and the elytra of beetles, exposed after a heavy gale.

A letter was read from Mr. T. B. Jordan, of the Museum of Economic Geology, *on copying Fossils by a galvanic deposit.*—In applying the method ordinarily used in electrotyping, some difficulty was experienced by the author in consequence of the irregular form of the fossils, parts of which would not relieve from the wax or plaster matrix in which the copper is afterwards deposited. Mr. J. therefore adopted a compound of glue and treacle, (used by printers for their inking rollers,) as the material of the moulds, the elasticity of which admits of its leaving the

adherent portions without breaking. The mixture is applied hot, and allowed to harden for twenty four hours, when it will come off without injuring the finest parts. The matrix thus prepared requires a strong varnish to protect the back and sides from the action of the liquid in which it is to be placed, and only one copy can be made from each matrix, but the impressions have none of the defects so apparent in those produced in the ordinary moulds. Different lights and shades may be given to the copper impressions, by a galvanic process, which the author considers capable of improvement, and application to other purposes. For a dark object on a light ground, the surface is brushed over with the argento-cyanide of potassium, giving it a silver face, which may be removed to the desired extent from the portions requiring to be dark, by a dilute solution of nitro-muriate of platinum. Other tints may be produced by using a solution of gold; and all may be considerably varied by changing the time during which each solution is allowed to act.

Prof. Owen communicated the second and concluding portion of his *Report on British Fossil Reptiles.* After some prefatory observations on the general nature and affinities of the recent and extinct reptilia, and the parts of the organization of the latter, which by their modifications afford the best character for their determination, the author proceeded to give a recapitulation of the leading peculiarities of the *Enaliosauri*, which formed the subject of the first part of his Report: and a brief summary of the results of the labors of previous geologists and anatomists, in the field which the second part of his Report had led him to explore. The first section of the Report was devoted to a description of a large reptile, the type of a new genus, to which the name *Pliosaurus* was given, and which formed the link connecting the Plesiosaurus with the crocodile family. The most conspicuous character of this genus consists in the cervical vertebræ, which are considerably shorter than those of the dorsal region: in this respect it differs from all recent Saurians, the vertebræ of which are characterized by retaining the same length throughout. From this cause, the neck of the Pliosaurus is short, compared with that of the Plesiosaurus, and approaches the condition of that region in the Ichthyosaurus. The more crocodilian proportions of the teeth also distinguish it from the Plesiosaurus, which in other respects it strikingly resembles. Remains of it have

been found in the Kimmeridge clay of Market Rasen, Weymouth, and Shotover. From differences in the relative proportions of these bones, Prof. O. considers them to have belonged to two distinct species of Pliosaurus. The remains of the Saurians of the crocodilian family, which complete the transition from the Enaliosaurians to the terrestrial lizards, were next noticed. The Report included descriptions of the fossil crocodiles in the British formations below the eocene tertiary strata to the oolite inclusive; and it was observed, that the extinct species deviated from the organic type of the existing crocodiles, in proportion as their remains occurred in strata geologically more remote from the present time. Not any of the species were identical with those now known to exist, and the modifications of structure in which they differed, were much more considerable than any which distinguish the skeletons of existing species from each other. The extinct species agreeing with the present crocodiles in possessing the ball-and-socket articulation of the vertebræ, in which the cavity is on the fore part, were first described. Of these, the *Crocodilus Toliapicus* is found in the London clay of Bracklesham, at Sheppey, and in beds of sand underlying the red crag at Kyson. The *Crocodilus cultridens* of the Wealden formation Prof. O. now considered sub-generically different from the crocodiles, and proposed for it the name of *Suchosaurus*. *Goniopholis crassidens*, another species from the Wealden, was described by Prof. O. as more completely mailed than any of the crocodile family; its remains occur in the Tilgate Forest and near Battle Abbey, and in the Purbeck limestone at Swanage. The next family of extinct crocodilians considered by Prof. Owen, are characterized by the biconcave structure of the vertebræ. Remains of the first of these, *Teleosaurus Chapmanni*, are abundant in the lias of the Yorkshire coast; and *T. Cadonensis*, which abounds in the oolitic formations near Caen, in Normandy, also occurs in the oolite near Woodstock, and at Stonesfield. Remains of two other species were noticed. The second genus, Steneosaurus, distinguished from the last by the sub-terminal position of the nostril, is from the Kimmeridge clay at Shotover, and from the oolite of Stonesfield. One of the most interesting specimens, exhibiting the form of the brain in a cast of that part, is in the Woodwardian Collection at Cambridge. A third division was for the first time described as occurring in British strata, possessing the ball-

and-socket articulation of the vertebræ, but with the position reversed, to which the name *Streptospondylus* has been given by Von Meyer. It has been found in the lias near Whitby, and the oolite near Chipping Norton. Prof. Owen next proceeded to describe the remains of some gigantic Saurians, ranging from the greensand to the oolite, and which rivalling the modern whales in bulk, may be presumed to have been of strictly aquatic, and probably of marine habits. They have the biconcave structure of the vertebræ, and the long bones show no trace of a medullary cavity. Of the first of these, named by Prof. Owen, *Cetiosaurus*, (described in report of *Proc. Geol. Soc.*) the vertebra and other bones, found in the lower oolite of Chipping Norton, belonged probably to an individual forty feet in length. Prof. O. has assigned to this species the name of *C. hypoölithicus;* and to another species the name of *C. epioölithicus*, remains of which, including a vertebra eight inches in length of body, and nine inches in transverse diameter, occurs in the Yorkshire oolite at White Nab. The ninth section of the Report was devoted to the description of a large marine Saurian, teeth of which were frequent in the chalk of Barnwell, and in Sussex, in the Folkstone galt, and the lower greensand near Maidstone. From the structure of the teeth, Prof. O. had assigned to it in his "Odontography," the name of *Polyptychodon*. Several bones of a gigantic Saurian, discovered by Mr. Mackeson in the greensand quarries near Hythe, were considered as probably belonging to the same genus. Of the genus *Mosasaurus*, only a few vertebræ have been found in the English chalk formation. Teeth, resembling those of the Mosasaurus, but differing in the elliptic form of the base of the crown in a transverse section, have been found in the chalk of Norfolk, and were described by the generic appellation, *Leiodon*. The report next proceeded to the account of the extinct species, which manifested, in the enduring parts of their organization, an intimate relationship with the numerous and varied tribes of the smaller and lower organized Saurians of the present epoch, to which the term *Lacertians* or *Squamate Sauria* were applied. Prof. O. observed that in this, as in the foregoing divisions of the Saurian order, the ancient world possessed very singular and also very gigantic species, which have now utterly perished, and have given place to carnivorous and herbivorous quadrupeds of more active habits and higher organization. The first fossils noticed,

were referred to a small genus of Lacertians from the chalk formation in Cambridge and Maidstone, to which Prof. O. had given the name *Raphiosaurus;* a portion of the lower jaw containing twenty two awl-shaped teeth, and another specimen consisting of twenty dorsal, two lumbar, two sacral, and a few caudal vertebræ, with the pelvic bones, were described. Part of the lower jaw, with teeth, of another lizard about as large as the Iguana, was described as occurring in the eocene sand under the red crag at Kyson. Remains of a Lacertian were next described from the celebrated oolite at Stonesfield. The structure of these bones indicates a close affinity to the scincoid lizards, the largest forms of which now exist in Australia, where they are associated with Araucariæ and Cycadeous plants, with living Clavagellæ, Terebratulæ, and Trigoniæ, and with the peculiar marsupial quadrupeds, the remains of all which forms of organized beings characterize the same stratum and locality as does the present extinct Lacertian. Prof. O. next proceeded to notice the more remarkable and gigantic forms of terrestrial Saurians of the same period, from the eocene tertiary to the oolites. Of these, the Megalosaurus, Iguanodon, and Hylæosaurus had been described by their original discoverer, Dr. Mantell, and by Dr. Buckland in his 'Bridgewater Treatise.' Prof. O. after pointing out additional peculiarities of structure discovered in specimens subsequently found, and the new localities from which these specimens were derived, observed that the name *Iguanodon*, by conveying the idea of a gigantic *Iguana*, created an erroneous idea of its affinities. No existing lizard differed more from the Iguana than did the Iguanodon, in the absence of the ball-and-socket joint of the vertebræ, and likewise in the structure of the teeth, which is characterized in the gigantic extinct herbivorous reptile by numerous parallel medullary canals. The femur of the Iguanodon, in the process continued from the inner side, near the upper third of the bone, deviates from all modern Lacertians, and approaches nearer the crocodiles, but surpasses them in the development of the ridge in question. A detailed description of the skeleton, founded upon nearly all the remains of the Iguanodon yet discovered, was next given; the form of the claw-bones of the Iguanodon, and especially of some enormous ones recently discovered with other bones at Horsham, was described, and from a comparison of these with other specimens from the Isle of

Wight, and with those preserved in the slab containing the Maidstone Iguanodon, Prof. O. stated it to be his opinion, that the animal did not possess the peculiarity of having the fore legs provided with compressed, and the hind legs with depressed claws, but that the narrow compressed curved claws occasionally found in the Wealden, belonged to another extinct reptile. This section of the Report concluded with a notice of all the British localities, and the strata in which those remains had been discovered. The anatomical peculiarities of the Hylæosaurus, another large extinct reptile of the Wealden clay, discovered by Dr. Mantell, were next described; and an account of the microscopical structure of the dermal bones was given. This remarkable reptile combines the sub-biconcave structure of the vertebræ, with crocodilian scutæ, and a plesiosauroid form of the scapular arch. The teeth not uncommonly found in the Wealden strata, formerly supposed to belong to the *Phytosaurus cylindricodon* of Jaeger, and more recently to the genus *Rhopalodon* of Fischer de Waldheim, Mr. O. showed to be quite distinct from both, and stated that if they were not the teeth of the *Hylæosaur*, they must belong to some unknown genus of Lacertine Saurian. The remains of the genera *Thecodon* and *Palæosaur*, from the magnesian conglomerates near Bristol, and of the genus *Cladeiodon* from the Keuper sandstone of Warwickshire, were next described. These were the most ancient of the Saurians yet discovered in Great Britain, and although they differ from modern Lacertians in the implantation of their teeth in distinct sockets, agreed with them in the form and structure of the teeth. The last genus of Saurians described, (*Rhynchosaurus, O.*) is new to science, and the remarkable peculiarities of its cranial anatomy, together with its vertebral characters, and the structure of the ribs, and some of the long bones, were given in detail. Characters of the crocodile, lizard and tortoise were combined in the forms and connexions of the bones of the skull, a nearly entire specimen of which had been transmitted by Dr. O. Ward, of Shrewsbury, to Prof. Owen from the Grinsill quarries of the new red sandstone, where the foot prints of a reptile agreeing in size with the Rhynchosaurus were not uncommon. Reasons were adduced showing the high probability that they were the foot prints of the Rhynchosaurus: they differ in shape from those of the Chirotherium, which were shown, in the concluding part of the Report, to be-

long to Prof. Owen's new genus *Labyrinthodon.* In the seventeenth section of the present Report, were described the remains of the flying reptiles (*Pterodactylus macronyx*) from Lyme Regis and the oolite of Stonesfield. Some remains of undetermined Saurians from the bone bed at Aust Passage, and other localities, were noticed. The nineteenth section contained an account of the fossil Emydes, Trionyces, and Cheloniæ, hitherto discovered in British strata. The *Chelonia Harvicensis*, and two new species (*C. breviceps* and *C. acutirostris*) from the eocene clay at Sheppey, were described; and the characters of a new genus, (*Cimochelys,*) the remains of which are found in the chalk near Maidstone, were given in detail.

The indications of Chelonian reptiles in more ancient strata were then noticed, and the femur of a tortoise, from the new red sandstone near Elgin, was described. The fossil reptiles of the order Ophidia, discovered by Mr. Owen in the London clay at Sheppey, have already been noticed; to these were added descriptions of a smaller species of *Palæophis*, from the eocene sand at Kyson, and of a much larger species not less than twenty feet in length, from the London clay at Bracklesham. The last section of the Report was chiefly devoted to the details of the determination of remains of the fossil Batrachians, identical with the so-called genera *Mastodonsaurus* and *Salamandroides* of the German Keuper, and on which the characters of the genus Labyrinthodon are based. Reasons were given, showing the high probability that the foot prints referred to the Chirotherium, were those of the Batrachian genus Labyrinthodon.

The following papers were also communicated:

On the Post-Tertiary Formations of Cornwall and Devon, by Mr. Bartlett.

On the stratified and unstratified volcanic products in the neighborhood of Plymouth, by Rev. D. Williams.

On the genus Cardinia of Agassiz, as characteristic of the lias formation, by H. E. Strickland.

On the discovery of organic remains in a raised beach, in the limestone cliff under the Hoe, at Plymouth, by E. Moore, M. D.

Account of the strata penetrated in sinking an Artesian well at the Victoria Spa, Plymouth, by Edward Moore, M. D.

Notice of the discovery of some fossils on Great Hangman Hill, near Combe Martin, North Devon, by Major Harding.

Sect. D. *Zoology and Botany.*

On the Geographical Distribution of the Animals of New Holland, by Mr. Gray.—Of the ninety four species of mammalia,

(belonging to thirty three genera,) which are found in Australia, it appears that fifty eight inhabit New South Wales, of which forty one are peculiar to it, and thirteen common to it, and other parts of the country; twelve species inhabit South Australia, six are peculiar, and six are common to other parts. Nineteen species inhabit Western Australia; twelve peculiar, and seven common. Five species inhabit the Northwest coast, all of which are peculiar to it; two species the north coast, one of which has not been found elsewhere. In Van Diemen's Land are found twenty one species; eleven are found only in that country, and ten common to it and the continent. One species is found in Norfolk Island, which is also found in New South Wales, but not in Van Diemen's Land.

On two remarkable marine invertebrata inhabiting the Ægean Sea, by Mr. E. Forbes.—These animals were taken in the harbor of Nousa, in the island of Paros, which is extremely rich in marine productions. The depth of the bay is generally from seven to ten fathoms; the bottom, sand and weed. Amongst the sandy heaps at the bottom of this bay are two new animals. The first, a zoophyte of the family Actiniadæ, which is free and vermiform, and which lives in a tube of its own construction,—a combination of characters hitherto unnoticed among the helianthoid polypes. The second is a tubicolar annelide, which lives in a strong gelatinous tube, bearing a remarkable analogy to the sac of certain entozoa. These two animals are noticed together, as in each case the peculiarity of the organization and habits is the result of a similar adaptation of form, in two very distinct tribes, to a similar locality.

On a new Glirine Animal from Mexico, by J. E. Gray.—This animal was brought from Mexico by Mr. J. Phillips, and is peculiar for having large cheek pouches which open externally on the sides of the cheek. This conformation has hitherto been observed only in four genera of glirine animals, which inhabit exclusively the northern half of the American continent, as the genera Saccophorus, Saccomys, Anthomys, and Heteromys. These cheek pouches are used by the animals to carry their food, as the monkeys of the Old World use their internal pouches, which are found between their cheek and the mouth. The first of these genera has been long known; and it has been believed that these cheek pouches hung out of the side of the cheek like pockets; but this

does not appear to be the case with the genus under consideration, or with the Anthomys, which was so called because F. Cuvier found their cheek pouches filled with flowers. If it were not for these cheek pouches, the animal before us might be taken for a Gerboa, with which it perfectly agrees in the softness and color of the fur, and in the length of the hind-legs and tail, which has a brush at the end, so that it may be at once distinguished from the other American genera, above enumerated, which either have an elongated scaly tail, like a rat, or a very short one, like a lemming. Mr. Gray is inclined to consider this animal as the representative of the genus (Dipus) Gerboa, which is confined to the more temperate part of Africa, as the genus Harpalotis is representative of the same genus in Australia. The combination of the forms and color of the Gerboa with the external cheek pouches of the pouched rat, at once marks this animal as a new genus, which I propose to call Dipodomys or Gerboa rat, designating the species after its discoverer, *D. Phillipsii.*

Col. Hamilton Smith read a paper *on the Colossal Sepiadæ.*— He detailed all that was known of the existence of animals of enormous size, inhabiting the ocean, belonging to the class of Cephalopods. However incredulous some naturalists might be regarding the existence of these animals, he had collected sufficient evidence to convince him that animals of a very large size belonging to this class now inhabited the waters of the ocean. The paper was illustrated by numerous drawings, and one was a sketch of the beak and other parts of an enormous Sepia, still preserved at the Museum of Haarlem, where they were seen by the author.

The following papers were also read:

Some inquiries on the Natural History of the Wheat Midge, Cecidomyia Tritici, by Prof. Henslow.

On the zoology of the county of Cornwall, by J. Couch.

On some species of European Pines, by Capt. Widdington, R. N.

On the existence of organic beings in mineral waters, by Dr. Lankester.

Report on the drawing up, printing and circulating of queries concerning the human race, for the use of travellers and others.

Report of the Committee on the growth and vitality of seeds.

On the habits of the eel, and on the freshwater fishes of Austria, by Capt. Widdington, R. N.

On animal exhalations as affecting plants, by Mr Ball.

On natural history as a branch of general education, by Mr. R. Patterson.

Scheme for yearly observations on the periodicity of birds, by M. de Longchamp, of Liege.

Comparative view of animal and vegetable physiology, by Mr. Bartlett.

Remarks on the Flora of Devon and Cornwall, by Rev. W. S. Hore.

Account of a Thylacinus, the great dog-headed opossum, one of the rarest and largest of the marsupiate family of animals, by Prof. Owen.

Account of two Peruvian mummies, by Mr. P. F. Bellamy.

On the varieties of the human race, by Dr. Caldwell.

Sect. E.—*Medical Science.*

The following papers were communicated to the Section.

Facts as yet unnoticed in the treatment of squinting, by Mr. J. V. Solomon.

General observations on the pathology and cure of squinting, by J. Butter, M. D.

A case of albuminous Ascites, with Hydatids, by Sir David Dickson.

Observations on a pustular disease hitherto undescribed by writers on diseases of the skin, by Dr. A. T. Thomson.

Report on poisons, by Dr. Roupell.

On the treatment of Rheumatism by opium, by Dr. Theophilus Thompson.

On Empyema, by Mr. Square.

On two fasciæ on the eyeball, by Mr. B. Lucas.

Some observations on a case of deafness, dumbness and blindness, with remarks on the muscular sense, by Dr. Fowler.

Sect. F. *Statistics.*

The following papers were communicated to the Section.

Statistics of Plymouth, Stonehouse and Devonport, by Mr. H. Woolcombe.

On the vital statistics of Sheffield, by a local committee.

Account of the Polytechnic School of Paris, by J. Heywood, Esq.

On the loan funds in Ireland, by Mr. H. J. Porter.

On the income of scientific and literary societies, and the amount paid for rates and taxes in the year 1840, by Mr. Ryland.

Account of the Central Statistical Commission in Brussels, by the Belgian government; from Prof. Quetelet.

Results of experiments on a system of small allotments and spade husbandry, by Mrs. Davies Gilbert.

On the agricultural products of Cornwall, by Sir C. Lemon.

Report of the Manchester Statistical Society, on the state of the working classes in the borough of Kingston-upon-Hull.

Statistics of education in the city of Bristol, by Mr. Fripp.

Comparative statement of the income and expenditure of certain families of the working classes in Manchester and Duckenfield, during the years 1836 and 1841, by Mr. W. Neild.

Account of the Monts de Piété of Rome, Paris, and other continental cities, by Mr. Porter.

On the stipends of the clergy of the Established Church in Scotland.

Address on the importance of keeping exact registers, in different districts, of facts in meteorology, physics, chemistry, botany, agriculture, zoology, human physiology and economy, by Prof. Quetelet.

On the economic statistics of Sheffield.

Sect. G. *Mechanical Science.*

Mr. G. Rennie read a paper on *the Propulsion of Vessels by the Trapezium Paddle-wheel and Screw.* The author gave an

account of the various experiments to which he had been led, on the propulsion of vessels by various forms of paddle-floats and by the screw. It was generally admitted that the paddle-wheel is the best means of propulsion with which engineers are at present acquainted, and various attempts have been made for its improvement. There are several objections to the square or rectangular floats, particularly the shock on entering the water, and the drag against the motion of the wheel on the float's quitting the water; both of which gave rise to considerable vibrations. He had been led, in considering the improvement of the paddle-wheel, to have recourse to nature; and the form of the foot of the duck had particularly attracted his attention. The web of the duck's foot is shaped so that each part has a relation to the space through which it has to move, that is, to the distance from the centre of motion of the animal's leg. Hence he was led to cut off the angles of the rectangular floats; and he found that the resistance to the wheel through the water was not diminished. Pursuing these observations and experiments, he was led to adopt a float of a trapezium or diamond shape, with its most pointed end downwards. These floats enter the water with their points downwards, and quit it with their points upwards, and then arrive gradually at their full horizontal action, without shocks or vibrations, and after their full horizontal action, quit the water without lifting it or producing any sensible commotion behind. After a great variety of experiments, he found that a paddle-wheel of one half the width and weight, and with trapezium floats, was as effective in propelling a vessel as a wheel of double the width and weight with the ordinary rectangular floats. The Admiralty had permitted him to fit Her Majesty's steam-ship *African* with these wheels, and he had perfect confidence in the success of the experiment. Another means of propulsion is the *screw*, which had been applied with success by Mr. Smith in the *Archimedes*. In examining the wings of birds and the tails of swift fish, he had been particularly struck with the adaptation of shape to the speed of the animals. The contrast between the shape of the tail of the codfish—a slow moving fish, and the tail of the mackerel—a rapid fish, is very remarkable; the latter going off to a point much more rapidly than the former. From these observations he was led to try a screw with four wings of a shape somewhat similar to these, but bent into a conical sur-

face, the outline being a logarithmic spiral. He found also that certain portions of these might be cut off without diminishing the effect. With respect to ascertaining the friction of the screw on the water, great difficulty exists; but he would refer to his experiments, published some years ago in the Philosophical Transactions, in which he measured the friction of the water against a body revolving in it, by the time which a given weight took to descend; this body consisted of rings, and he found that the friction or resistance through the water did not increase in proportion to the number of rings.

The following papers were also communicated.

Report on railway constants, by Dr. Lardner.

Remarks on the connexion which exists between improvements in pitwork and the duty of steam-engines in Cornwall, by Mr. Enys.

On Mr. Truscott's plan for reefing paddle-wheels, by Mr. Chatfield.

On a plan of disengaging and reconnecting the paddle-wheels of steam-engines, by Mr. J. Grantham.

On a floating breakwater, by Capt. Taylor, R. N.

Further report of the committee on the forms of vessels, by Mr. J. S. Russell.

On an improved sight for rifles and other fire-arms, by Mr. C. T. Coathupe.

On Capt. Couch's chock channels, by Mr. Snow Harris.

On Arnott's stove, and the construction of descending flues, and their application to the purposes of ventilation, by Mr. J. N. Hearder.

Report of the committee on railway constants, by Mr. Edward Woods.

On the granite quarries of Dartmoor, and their railways and machinery, by Mr. W. Johnson.

Report of the committee for applying a principle of Dynamometrical admeasurement, invented by M. Poncelet, to the construction of a permanent indicator for steam-engines.

On a system of trussing for the roadways of suspension bridges, by Mr. Rendel.

On the Plymouth breakwater, by Mr. Wm. Stuart.

Art. XIV.—*An Astronomical Machine, the Tellurium;* by Edwin C. Leedom, M. D., of Plymouth, Penn.

This is a machine for representing the motions of the earth and moon. The earth, whose axis has its proper obliquity to the ecliptic and keeps its parallelism, revolves round the sun in an ellipsis similar to the natural orbit, and moves with such a velocity that an imaginary line joining the centres of these two bodies, the latter being situated in one of the foci of the orbit of the former, describes equal areas in equal times. The diurnal rotations of the planet are also shown, each complete turn on its axis

being made in a sidereal day, or 23h. 56m. 4s. The moon moves eastward round the earth and completes a sidereal revolution in 27d. 7h. 43m.; its nodes shift round contrary to the order of the signs, and its apogee has its direct motion eastward, the former completing a sidereal revolution in 18.6 years, and the latter in $8\frac{1}{2}\frac{7}{6}$ years.

In contriving this machine I have availed myself somewhat of the inventions of other artists. To effect the unequable motion of the earth in its orbit, I have had recourse to a combination of elliptical wheels similar to that used by Dr. Desaguliers in his Cometarium. There is a little Planetarium described in Ferguson's Astronomy, in which the parallelism of the earth's axis is preserved in the same manner as in this. But this machine, independently of the elliptical orbit and unequable motion of the earth, is very different from that, as will be apparent to any one who may compare them.* (See Brewster's Ed. Ferg. Astron. Vol. II, p. 6.) Although these particular parts are the inventions of preceding artists, still I think I may venture to assert, that this machine, considered as a whole, constitutes a new combination in mechanics.

In Plate IV, this machine is represented as it would appear to an eye situated directly above it. Plate V exhibits a lateral view of the wheelwork. In either plate the ball W represents the sun, the ball U the earth, and V the moon: *k* is an index for showing the place of the moon's ascending node, *e* is another index for showing the place of its apogee, and *n* is a winch by which the machinery is moved. The earth is surrounded by a little brass ring *s*, which is set upon four pillars *t t*, and has the signs of the zodiac marked upon it. Upon this ring, which moves with the earth and keeps its parallelism, the geocentric places of the sun, moon, its ascending node and apogee, can be seen. 1 2 3 4, Plate V, is a wooden frame, in the top of which are two equal elliptical grooves similar to the earth's orbit, and which have their foci all situated in one straight line. Within the frame are two elliptical wheels, K and L, which are of the same size and eccentricity as these grooves, each wheel having its axis

* About fourteen years ago I made the first machine of this kind. At that time and for several years after, I believed myself to be the original inventor of this mode of preserving the parallelism of the earth's axis, but I was at length undeceived by a perusal of Ferguson's book.

situated in one of its foci. The axis of the wheel L also carries a large circular wheel M; next to which is placed a pinion N, upon the upper end of whose axis the winch *n* is fixed. Q is a stout metallic axis of the same size as that which carries the wheel K. These two axes pass perpendicularly through the boards 1 and 3, the upper part of each axis where it comes through the board 1 being situated in one of the foci of one of the elliptical grooves. Upon the upper ends of these axes two arms *h* and *i* are tightly fixed. Two other arms R and S are fixed upon their lower ends so as to be perpendicular to *h* and *i*. T is a narrow metallic plate which is connected with the arms R and S by two movable joints: this plate assists in regulating the motion of the machine. Into the arms *h* and *i*, are inserted two axes *f* and *g*, which pass up through a movable frame 5 6 7 8 and turn freely within it. The lower ends of these axes project into the elliptical grooves in the board 1 and slide along these grooves when the machine is in motion, the arms *h* and *i* being so contrived as either to lengthen or shorten according as the distance of the groove from its focus increases or diminishes.

The movable frame 5 6 7 8 contains a number of wheels, which serve to rotate the earth on its axis and give motion to the moon, its nodes and apogee. A metallic supporter Y has inserted into it a long and narrow socket, which passes up through a hole in the plate Z. Upon the upper end of this socket a small brass arm *a* is fixed, which holds a pinion *o*, whose axis forms an angle of 23½ degrees with the perpendicular, and carries the earth U. C is a pinion whose axis passes up through this socket and is surmounted by a very small wheel whose teeth act upon the leaves of the pinion *o*. D, F, and H, are three wheels, each of which is fixed upon a separate socket. The socket of the wheel D turns upon the socket which is fastened into the supporter Y. The socket of F turns upon the socket of D; and the socket of H turns upon that of the wheel F. Upon the upper end of the socket of D a small circular brass plate *c* is fixed, into which, near its edge, is inserted a small flattened socket, through which passes a flattened wire which carries the moon V. The lower end of this wire rests on another circular plate *d*, which is fixed upon the socket of the wheel F and has an oblique position, forming an angle of 5½ degrees with a horizontal plane passing through its centre. This wire is kept constantly applied to the

plate *d* by means of its own gravity, and slides along this plate as *c* turns round, the wire alternately rising and falling in its socket; consequently the orbit in which the moon V moves must always be parallel to the plate *d*, and form an angle of 5½ degrees with the plane of the ecliptic. The index *e*, which points to the moon's apogee, is fixed upon the socket of the wheel H. The axis *g* carries four wheels A, E, G, and I, which all turn as one wheel. Next to the wheel A is placed the pinion of a wheel B, whose teeth act upon the teeth of a small wheel *p*, which transmits motion to the pinion C. The teeth of the wheel E act upon the teeth of the wheel O, whose axis also carries a wheel P, which gives motion to the wheel D. The teeth of the wheel G act upon the teeth of the wheel F; and lastly, motion is transmitted from the wheel I to the wheel H by means of an intervening wheel *r*.

When the winch *n* is turned by a steady hand, the leaves of the pinion N act upon the teeth of the large circular wheel M, and turn it and the elliptical L on their common axis with an equable motion. The teeth of L at the same time act upon those of the wheel K. As K turns, the arms *h* and *i* both move in the same direction and carry the movable frame 5 6 7 8 parallel to itself, over and over the top of the large stationary frame 1 2 3 4. The earth U is carried along with the moving frame, and has the parallelism of its axis also rigidly preserved. As the ends of the axes *f* and *g* slide round in the elliptical grooves, in the board 1, it is apparent that the orbit described by the earth U, must be an ellipsis of the same size and eccentricity as either of these grooves. When the earth is in its perihelion, as represented in the drawing (Plate V,) that part of the circumference of the elliptical wheel L, which is farthest from its axis and has the greatest velocity, is applied to a part of the circumference of K which is nearest to the axis of the latter wheel, consequently the earth must have its quickest motion. When the earth comes to its aphelion, these elliptical wheels have a reverse position with respect to each other, which gives the earth its slowest motion. These elliptical wheels working together in this manner, give the earth U the same unequable motion in its orbit, that the real earth has in nature.

The wheels A, E, G, and I, all make one complete turn on their common axis *g* during an entire revolution of the earth

round the sun. The wheel A contains 293 teeth, and the pinion which belongs to the wheel B contains 8 leaves, consequently B must make $36\frac{5}{8}$ turns during one turn of the wheel A. The wheel B contains 80 teeth, and the pinion C contains 8 leaves; consequently C must make ten turns during one turn of the wheel B, and ten times $36\frac{5}{8}$ or $366\frac{1}{4}$ turns during one revolution of the wheel A, that is, in one year. The little wheel upon the upper end of the axis of the pinion C contains the same number of teeth as the pinion *o*, therefore the earth must turn on its axis in the same time as the pinion C; it must make $366\frac{1}{4}$ diurnal rotations in a year, each rotation being performed in a sidereal day or 23h. 56m. 4s. The wheel E contains 167 teeth, and the wheel O contains 25 teeth. Consequently O, and also the wheel P, must make $6\frac{17}{25}$ turns during one turn of E. The wheel P contains 72 teeth, and the wheel D contains 36 teeth, consequently D must make two turns during one turn of P, and twice $6\frac{17}{25}$ or $13\frac{9}{25}$ turns during one turn of the wheel E, or in one year. As the circular brass plate *c* is fixed upon the socket of the wheel D, this plate must turn with the wheel and carry the moon V $13\frac{9}{25}$ times round the earth U in a year, which is equal to the number of the moon's sidereal revolutions in this time. The teeth of the wheel G act upon the teeth of the wheel F. The wheel G contains 20 teeth, and the wheel F contains 372 teeth, consequently G must make $18\frac{6}{19}$ turns, while F turns once round. As the wheel G makes but one turn on its axis in a year, the wheel F must require $18\frac{6}{19}$ years to perform a revolution. The oblique plate *d*, to which the moon's orbit is parallel, being fixed upon the socket of the wheel F, the plate must turn with this wheel and carry the moon's nodes round contrary to the order of the signs, so as to perform a sidereal revolution in $18\frac{6}{19}$ years. The teeth of the wheel I act upon the teeth of the wheel *r*, which, as before stated, transmits motion to the wheel H. The wheel I contains 20 teeth, and the wheel H contains 177 teeth, consequently I must make $8\frac{17}{20}$ turns in order to turn F round once. As the wheel I makes only one turn in a year, the wheel H must be $8\frac{17}{20}$ years in performing a revolution. The index *e*, which is fixed upon the socket of H, must turn with this wheel and also perform a revolution in $8\frac{17}{20}$ years, which is the time in which the moon's apogee performs a sidereal revolution; therefore, this index will show the proper motion of the apogee.

This machine being rectified by the astronomical tables for any particular time, if the winch *n* be then turned from right to left, the machine will exhibit the vicissitudes of day and night, variety of seasons, new and full moons, eclipses, anomalies of the sun and moon, or year after year.

As the earth has the same unequable motion in its elliptical orbit that the real earth has, this machine will show the sun's true place correctly for a great length of time.

A table showing the dimensions of the wheels of the Tellurium, number of teeth, &c.

Wheel A, 293 teeth, 12 teeth in an inch of circum. Diam. 7.77 inches.
Wheel B, 80 teeth, 8 teeth in an inch of circum. Diam. 3.18 inches.
Pinion of wheel B, 8 leaves. Diam. $\frac{21}{100}$ths of an inch.
Wheel *p*, 24 teeth, 8 teeth in an inch. Diam. $\frac{95}{100}$ths of an inch.
Pinion C, 8 leaves. Diam. $\frac{31}{100}$ths of an inch.
Wheel E, 167 teeth, 8 teeth in an inch of circum. Diam. 6.64 inches.
Wheel O, 25 teeth, 8 teeth in an inch. Diam. $\frac{99}{100}$ths of an inch.
Wheel P, 72 teeth, 8 teeth in an inch. Diam. 2.86 inches.
Wheel D, 36 teeth, 8 teeth in an inch. Diam. 1.43 inches.
Wheel G, 20 teeth, 12 teeth in an inch of circum. Diam. $\frac{53}{100}$ths of an inch.
Wheel F, 372 teeth, 12 teeth in an inch. Diam. $9\frac{86}{100}$ inches.
Wheel I, 20 teeth, 8 teeth in an inch of circum. Diam. $\frac{79}{100}$ths of an inch.
Wheel H, 177 teeth, 8 teeth in an inch. Diam. $7\frac{4}{100}$ inches.
Wheel *r*, 40 teeth, 8 teeth in an inch. Diam. $1\frac{59}{100}$ inches.
Elliptical wheels K and L. The longer diameter of each wheel, 10 inches. Distance between the two foci $\frac{17}{100}$ths of an inch. Both of these wheels contain the same number of teeth.
Wheel M, 280 teeth, 8 teeth in an inch. Diam. $11\frac{14}{100}$ inches.
Pinion N, 16 leaves.
Elliptical grooves in the board 1. The longer diameter of each groove, measuring from the middle of the groove on one side to the middle of the groove on the opposite side, 10 inches. Distance between the two foci $\frac{17}{100}$ths of an inch.

ART. XV.—*Abstract of a Meteorological Journal, for the year* 1841, *kept at Marietta, Ohio, Lat.* 39° 25′ *N., Lon.* 4° 28′ *W. of Washington City;* by S. P. HILDRETH, M. D.

Months.	THERMOMETER. Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Rain and melted snow. Inches.	100ths.	Prevailing winds.	BAROMETER. Maximum.	Minimum.	Range.
January,	32.47	59	−4	10	21	3	87	N. W., W., S. W.	30.10	28.98	1.12
February,	33.00	57	8	14	14	1	31	N. W., S. W.	29.70	28.90	.80
March,	42.33	80	16	18	13	3	42	S. W., E., S. E.	29.80	28.95	.85
April,	46.66	82	24	17	13	5	80	N., S. W.	29.80	28.75	1.05
May,	60.20	92	30	26	5	3	37	N., S. W., S.	29.50	29.05	.45
June,	75.27	94	50	22	8	4	30	N., S. W., S.	29.52	29.20	.32
July,	71.43	92	53	21	10	3	50	S., S. W., N.	29.60	29.25	.35
August,	70.53	90	51	23	8	3	17	N., S., S. E.	29.65	29.30	.35
September,	66.28	91	51	20	10	3	37	S., S. E., N.	29.63	29.05	.58
October,	48.44	78	24	18	13	1	83	S., S. E., N.	29.75	29.05	.70
November,	43.20	79	21	10	20	3	50	W., S. W.	29.75	28.90	.85
December,	36.33	60	13	6	25	5	38	S. W., S. E.	29.75	28.78	.97
Mean,	52.18			205	160	42.82					

Remarks on the year 1841.

The past year has not been remarkable for any striking changes in the temperature, or commotion in the elements. The mean heat for the year is very near that of this climate for a series of years, being 52.18°. The distribution varies very considerably, as it is divided amongst the seasons; some springs being cool, others warm; so also with summers and autumns, while the general amount varies but little in different years. The winter was comparatively mild, the mercury sinking below zero only on two mornings during this period. The mean is 32.51°, being two degrees cooler than that of 1840. There fell but eight or ten inches of snow at different times. The Ohio river at Marietta was closed only for a few days by the ice—from the 3d to the 8th of January, but navigation was impeded by floating ice as early as the 21st of December. In February, there was some severe weather, from the 10th to the 15th of the month. The mercury was at 8° above zero on the morning of the 11th, and only 12° above at noon; at this period we often have the coldest weather in the year. The Ohio was again filled with floating ice, and for a few days boats ceased to run; after the 20th of the month,

the river was free from ice until the breaking up of the Alleghany river. The Ohio has at no time been over its banks; on the 30th of March it was nearly "full banks."

The mean temperature for the spring months, was 49.73°, which is several degrees below that of 1840, or that for the average heat of this place. April was uncommonly cool, being only 46.66°, while the same month in 1840 was 56.57°—a difference of nearly 10°. The consequence was the retardation of the blossoming of fruit and other trees for several days past the usual period. In 1838, the apple bloomed on the 17th of April, and in 1840 on the 15th; while this year it was not in full bloom until the 30th of that month, and did not entirely shed its blossoms before the 20th of May. Other trees were retarded in nearly the same proportion. From the 10th of May to the 11th of June, there fell but little rain, being only about half an inch. The lack of moisture at this season of the year, when the roots of cereal plants are usually in their most vigorous state, was seriously felt in the crops of wheat and grass, especially the latter, which afforded but a small crop compared with other years. Indian corn, owing to the cool and dry weather in May, did but barely appear above ground, as late as the 10th of June, and many farmers feared an entire failure of the crop; but refreshing showers after that period, and the heat of June, soon awakened its dormant powers, and an average crop was obtained in this part of Ohio; while west of us on the Scioto river, this crop was nearly a complete failure, the drouth continuing in the valley of that stream till late in June.

The mean temperature of the summer months was 72.41°; which is nearly 2° above that of 1840. During these months, there was a seasonable supply of rain, and vegetation was healthy and vigorous. The last of June, when the wheat crop had nearly attained its growth, during a period of wet sultry weather, the cuticle of the stem was attacked with a rust or mildew; it appeared to arise from an exudation of the sap, like a heavy dew, on the leaves of forest trees; after a few days, the stalks were covered with minute reddish spots of mould, raising quite a cloud of dust when disturbed by the reapers. This took place while the grain was in the milk; the consequence was, a lack of those nutritious juices which perfect the kernel, and the grain was shrunken and tight, affording an undue proportion of bran when

manufactured into flour. Very few fields in the southern portion of Ohio escaped this calamity; while the crops of this grain in the northern part of the state being later in ripening, suffered much less.

The summer was attended with no tornadoes, or violent gusts of wind, to do any material damage.

The mean temperature of autumn was 52.80°, which is about 2½° warmer than that of last year. The warmth of this period gave the Indian corn full time to ripen before the appearance of frost, the first of any severity being on the 18th of October.

Crops of potatoes, beans, and oats, were very good; that of sweet potatoes was uncommonly fine. Fruit was not abundant, being injured by the frosts of May; apples were plentiful in some districts, while in others within a few miles, they were an entire failure. Peaches were quite prolific on the hills back from the river, and near the Ohio, also, if located on the top of a high hill. Trees in this situation rarely fail of producing fruit; and those who set new orchards now, look out for exposures of this kind.

The amount of rain for this year has been $42\frac{8\,0}{1\,0\,0}$ inches, which is about the average for this climate. It is $3\frac{7\,5}{1\,0\,0}$ inches greater than that of 1840, and nearly a foot greater than that of 1839, which was an uncommonly dry year.

A brilliant aurora was observed about the 18th of November, but I did not happen to witness it.

Marietta, Ohio, January 5, 1842.

ART. XVI.—*The Glacial Theory of Prof. Agassiz;* by CHARLES MACLAREN.*

THIS is perhaps the fittest term to designate the novel opinions of M. Agassiz. Glaciers are properly long narrow masses of ice filling the bottom of Alpine valleys, but M. Agassiz thinks that sheets of ice, such as are met with in Greenland, covered the whole surface of Europe, and all Northern Asia as far as the Caspian Sea. This conclu-

* This article is republished from a little tract which Mr. Maclaren had the kindness to send us, entitled "The Glacial Theory of Prof. Agassiz of Neuchatel, being an outline of facts and arguments adduced by him to prove, that a sheet of ice enveloped the northern parts of the globe at a recent geological epoch; by Charles Maclaren." As it is the best review of the subject which has met our eye, we deem no apology necessary to our readers for republishing it here.—EDS.

sion, which has been adopted in whole or in part by Professor Buckland, Mr. Lyell, and other eminent geologists, has been deduced from a careful study of the phenomena attending glaciers, some of which are of so marked and peculiar a kind, as to afford satifactory evidence of their ancient existence in situations where none are now seen. The Swiss philosopher advanced in his opinions step by step. He first satisfied himself that in the Alpine valleys where glaciers still exist, they once rose to a higher level, and extended farther down into the low country than they now do. Next he discovered indications of their former existence on Mount Jura and over the whole Swiss valley; and connecting these with similar indications found in the Vosges, the Scandinavian Mountains, and elsewhere, and with the well known fact of sheets of ice covering the northern shores of Siberia and entombing the remains of extinct species of animals, he came to the conclusion, that at a period, geologically speaking, very recent, all the old world north of the 35th or 36th parallel, had been enveloped in a crust of ice. Whence the cold came which produced this effect, and why it afterwards disappeared, are questions he did not feel himself bound to answer, but which might, perhaps, be answered hypothetically. In reality, if we suppose the Northern Atlantic from the 39th parallel filled up and converted into dry land, it is extremely probable that Britain would have the ice-bound climate of Labrador, with which it corresponds in latitude; and the conversion of the said land into sea would bring back the order of the seasons which we now enjoy. Even though M. Agassiz's opinions should not be fully established, they still afford us a new geological agent of great power and widely applicable, which may help us to an explanation of some phenomena very difficult to account for with our existing means of information.

Form, Magnitude and Composition of Glaciers.—The subjoined figure is not a section, but a view of a glacier as it would present itself to an eye raised considerably above it.

Fig. 1.

a b, (fig. 1,) The glacier: *a* represents one of the forms of its surface, in which it is bristled with cones of snow or ice, called *aiguilles* or needles: *b* is the other and more usual form of the surface, consisting of narrow ridges or corrugations, like waves fixed by frost.

c d, *Lateral moraines*, consisting of long lines of boulders and gravel, which having been detached by frost, rain, lightning, or avalanches, from the rocks flanking the valley, settle on the two sides of the glacier.

The heat reflected from the rock fuses a portion of the ice nearest it, or hastens the evaporation, rendering the sides of the glacier a little lower than the middle, and giving the mass a convex shape. The fragments rest in the hollows thus produced, and assume the form of the roof of a house, one side sloping down to the rock and the other to the ice.

e f, The *terminal moraine*, a line of boulders and gravel at the lower end of the glacier, which it pushes before it when advancing, and leaves behind it when retreating. In the latter case it looks like a low mound or barrier across the valley. The terminal moraine is a continuation of the two lateral, but they are not always found united.

r r, The rocks forming the flanks of the valley.

In the higher parts of the Alps, the perpetual snow forms vast extended masses joining the peaks and ridges, and these, called *mers de glace*, or "seas of ice," exhibit scenes of grandeur and desolation which have been the wonder of travellers. The glaciers are branches or offshoots from these, filling the valleys which descend from the higher regions to the lower. Glaciers pass down sometimes to so low a level as 3000 feet above the sea in Switzerland; but they do not originate at a lower elevation than 7000 feet, and they rarely exist on isolated mountains, whatever be their height. In the upper part they consist of granular snow, called *nevé* in the Alps, which is changed into minute crystals of ice by the infiltration of water, arising from the outer portion of the snow being melted by the sun. As we descend from the higher end of the glacier, the crystals, which are rather irregular fragments, become gradually larger. Towards the lower end they are from half an inch to an inch and a half in diameter, and in some rare cases three inches. If a section of the glacier is exposed, the upper strata (for it is generally stratified) are found to be full of cells, and its substance becomes gradually more compact downward, the lowest part being the most solid. The strata are thick at top, thinner in the middle, and disappear towards the bottom. Glaciers contract in breadth and depth as they descend; one a league broad at the head will sometimes be only 150 or 200 yards at the foot. The thickness varies from 80 to 100 feet at the lower, and from 120 to 180 feet at the higher end. M. Agassiz adopts these measures from Hugi, and seems to reject the notion of older writers, that some glaciers are 500 or 600 feet in depth. Glaciers are of all lengths—from 100 yards to 15 miles.

Every glacier discharges a stream from a vault in its lower end in summer, which disappears in winter, except in some cases, where the water is believed to come from deep springs, with a temperature sufficiently high to keep their channels open.

There are numerous open rents or fissures (called *crevasses*) in every glacier, caused partly by the uneven surface over which the glacier glides in its downward motion—partly by the unequal expansion of the upper and under strata of ice. These fissures are of all widths—from a quarter of an inch to thirty feet or more; they are largest and most numerous at the sides, but sometimes extend completely across; they occasionally reach from top to bottom, but more frequently stop at a certain depth. Their direction is generally across the glacier, but they often become oblique at the sides, as the ice moves faster there than at the middle; and hence, viewed on the great scale, they present a curved or arched appearance, with the convexity turned towards the head of the glacier. The fissures are largest and most numerous at the lower end, and in the parts which are much inclined. In a steep valley, a glacier, with its wave-like ridges, its bristling cones, and the pointed rocks piercing its surface here and there, has been aptly compared to a cataract stereotyped.

The cones or needles of ice, as at *a*, figure 1. are thus accounted for by Agassiz: The glacier, in passing along a valley whose bottom is very uneven, breaks into numerous vertical prisms; and the summits of these, having their angles wasted away by the sun's heat and evaporation, gradually assume the conical shape.

Glaciers descend into regions where the annual temperature is eight or nine degrees above the freezing point; and, to use the words of Cox, there are localities in Switzerland where you may almost touch growing corn with the one hand, and the ice of the glacier with the other. They of course waste away at their lower end rapidly in summer, partly by fusion, and partly by huge fragments of the ice falling off, in consequence of the upper beds expanding faster than the lower, till the outer mass loses its balance and topples down.

Motion of Glaciers.—The geological action of glaciers depends chiefly on their motion, the true cause of which has been clearly ascertained for the first time by M. Agassiz. Previous writers on the subject, including the celebrated Saussure, attributed the motion of the glacier to gravitation, or the tendency of the mass of ice to descend by its weight from the upper part of the valley to the lower. This explanation accounted very imperfectly for the phenomena, and the opinion of Agassiz, deduced from a careful attention to facts, is now almost universally adopted. He considers the motion of the glacier as the consequence of expansion, and this expansion operates chiefly in the direction in which least resistance is experienced, that is, along the valley downward, and is caused by the congelation of infiltered water. The influence of the sun and of warm winds melts part of the upper surface, and the water so produced percolates into the spongy mass,

where it is soon frozen, and in freezing expands, according to a well known law. The upper strata, imbibing more water than the lower, dilate in a greater degree, but the lower strata, in dilating, carry the upper with them, and thus produce rents or *crevasses*. Again, the flanks of the glacier imbibe more water than the middle, and by their greater expansion give a curved form to the *crevasses*; and the lower end imbibes more water than the upper, in consequence of the more frequent rains and alternations of frost and thaw. Besides, as the upper end of the glacier, in expanding, pushes the rest before it, the accumulated effect of the whole expansion falls upon the lower end, which is found to travel quickest. The motion, too, is most rapid in summer, and nearly ceases in winter, in consequence of the water being then constantly frozen. From the effect of this internal movement of its parts, the glacier creeps along slowly but surely. In 1827, M. Hugi constructed a hut on the glacier of the Aar, at the foot of a fixed rock called *Im Abschwung*. It was found that the hut had receded 2200 feet from the fixed rock in 1836, and 4400 in 1840, showing that it had advanced about 250 feet per annum in the first nine years, and 550 in the four last. Taking summer and winter together, its motion had been about eight inches per day in the first period, and eighteen inches in the second. In glaciers which are much inclined, the motion is more rapid than this.

Polished and Grooved Surface of Rocks.—The glacier in its course downward carries with it the fragments of rock, gravel, and sand which lie under it. These adhere to the ice, or are embedded in it, and as the mass glides slowly along, they abrade, groove, and polish the rock, and the larger masses are reciprocally grooved and polished by the rock on their lower sides. The effects of this abrasion on the bottom of the valleys may be conceived from the pressure applied. A cubic yard of sandstone weighs two tons, and if we assume the average density of glacier ice to be two-thirds of that of common river ice, the pressure upon each square yard of rock at the bottom of a glacier 100 feet deep, will be equal to about sixteen tons, or the general pressure will be as great as would be produced by a bed of sandstone twenty four feet thick. Thus the various materials under the ice are pressed against the rock with an enormous force, while an equally great force of another kind, produced by the congelation of water, propels them downwards. The sand, acting like emery, polishes the surface; the pebbles, like coarse gravers, scratch and furrow it; and the large stones scoop out grooves in it. Portions of these substances, and of the rock too, are ground to the state of fine clay, and the whole of the movable matter, stones, pebbles, sand, and clay, are in course of time thrown out at the lower end of the glacier, where they form the terminal moraine.

The ice, in consequence of its tendency to dilate, and its numerous fissures, accommodates itself to the sinuosities of the rocks which confine it, cutting off the smaller projections, and rounding and polishing the larger, which assume the form of domes, and were termed *roches moutonnées* by Saussure. Agassiz's eighth plate gives some fine examples of these rounded swells. Owing to the immense pressure, the included pebbles of conglomerates, and the hardest veins in veined rocks, are cut away to the very same level with the softer parts which envelop them.

Thus, one of the marks by which the ancient existence of glaciers can be detected in situations where they are no longer seen, is the polished, striated, or grooved appearance of the rocks. Sometimes it is very distinct, but in many cases it is not visible, because the surface of most rocks wastes away by disintegration or decomposition, unless it is well protected by a covering of clay or turf. The most satisfactory specimen near Edinburgh, is in the quarry on the south side of Blackford Hill, at a place laid open a few years ago, where the rock leans forward, forming a sort of vault. The surface of the clinkstone here, for a space of ten or twelve feet in length, is smoothed, and marked by *striæ* or scratches in a direction approximating to horizontal. We accompanied M. Agassiz to the spot about two months ago; he had expressed doubts as to some other supposed marks of glacial action near this city, but on seeing those at Blackford Quarry, he instantly exclaimed—"That is the work of the ice." On the top of Salisbury Crags, at a quarry about two hundred yards from their south extremity, the polishing is very well seen at intervals over a space of twelve or fifteen feet just at the edge of the precipice; and *striæ*, running east and west, will also be discovered here by an eye accustomed to observe them, though they are much less distinct than at Blackford Hill. In quarrying the Crags at this spot, the rock had been cut back about one hundred and twenty feet from what was originally the edge of the precipice, and this part, which had been well protected by the turf, was only exposed about 1822 or 1823. We have little doubt that similar appearances would be presented if other parts of the greenstone, equally distant from the edge of the precipice, were *newly* laid bare. Parts of the north end of the Castle-rock are also curiously polished, and the groovings on the western slope of Corstorphine Hill, described many years ago by Sir James Hall, are well known. We have observed similar marks of abrasion at Craigleith Quarry, Craigmillar Hill, and elsewhere.

These marks of abrasion, both on rocks *in situ*, and on boulders found in the soil, have been usually attributed to the action of the currents of water, rolling along stones and gravel, an explanation felt not to be satisfactory, but adopted for want of a better. It is admitted that

rocks in the channels of rivers are often worn smooth; but Dr. Buckland contends, and apparently on good grounds, that straight parallel *striæ* and grooves never are, and cannot possibly be, produced by the action of gravel and stones in a stream. The abrading material, say a fragment of rock, if it rolls along, will perhaps make occasional indentations, or now and then an irregular scratch; but it cannot produce straight, parallel, continued *striæ* or grooves, unless held fast by some substance which prevents it from rolling, and gives its motion a determinate direction, as the cutter in a grooving plane is kept in a fixed position by the wood. Now, the ice of a glacier (or iceberg) is an agent which answers this purpose admirably; we see that it actually produces the effects described; we know no other agent capable of producing them; and it is therefore inferred that where well defined *striæ* or grooves are found on rocks, we have evidence of the former existence of moving masses of ice.

Moraines.—These afford other evidence of the ancient existence of glaciers after they have disappeared. Long terraces or banks of gravel are occasionally found on rocks forming the sides of valleys, high above the bottom, and where the surface they rest on is much inclined. Geologists have felt the difficulty of accounting for these deposits. Their situation is inconsistent with the idea that they were formed by running water; neither could they be deposited on the margin of lakes, because their parts are often found not to be on the same level. Again, they are found stretching like bars across the mouths of valleys, in situations from which a great current, so far from depositing them, would have swept them away, if they had previously existed. Now, both kinds are well accounted for on the supposition that they were the *moraines* of glaciers; those on the sides of the valley being *lateral*, and those barring up its mouth being *terminal* moraines.

Again, we sometimes find one or two long ridges of gravel stretching through a wide valley lengthwise or obliquely, without discovering any thing in the shape of the valley to indicate why the current, if water was the agent, should have accumulated the movable matter here, rather than spread it over the surface. This also is explained, if we assume that it was a *medial* moraine. When one valley opens into another, the two lateral moraines on the inner sides unite, and the compound glacier, besides having a line of blocks and gravel on each side, has a third stretching along the middle, and which is therefore called a *medial* moraine. There are examples in Switzerland of glaciers with three, four, or six medial moraines. Now, were the glacier to melt away, owing to a change of climate, these three, four, or six medial moraines would form as many ridges of gravel running along the bottom of the valley, or obliquely through it, and would resemble deposits occasionally seen in this country.

When a compound glacier is long, the different moraines, lateral and medial, sometimes become blended in their progress downwards, and spread out into a broad sheet; and if the ice were to disappear, we should find the whole bottom of the valley at this part covered with a confused assemblage of fragments of rock. This is also a deposit occasionally met with in Scotland.

The materials of moraines are not stratified, but huddled together in confusion. The fragments are generally somewhat rounded by mutual attrition, but some are angular. They may be distinguished from the banks of gravel formed at the margin of lakes by their internal structure, by the difference of level between their distant parts, and also by their form.

We are not quite sure of the precise shape of *terminal* moraines, but the terms employed by Agassiz (*digues ou remparts*) lead us to suppose that they form long mounds with rounded sides. Like the others, they are not stratified internally; but, from the manner of their formation, they contain more finely triturated matter, namely, clay, sand, and small gravel. Agassiz seldom gives precise measurements; but he mentions one terminal moraine, (that of Viesch,) which is thirty feet high, and much more in breadth. Glaciers sometimes advance for a term of years, and then retreat for another term. When a glacier is retreating, it forms a new terminal moraine every year, and when it again advances, it pushes the more recent ones before it till the whole are blended into one mass. Now, if the disappearance of the glaciers took place gradually, as it seems most reasonable to suppose, we ought to find in the lower end of some of our valleys a series of little transverse mounds, like x, y, in figure 5, below.

Lateral moraines increase in size towards the lower end of the valley, and for an obvious reason: The fragments which fall at the head of the valley are slowly carried downwards by the glacier in its course, and they are joined in their progress by those which fall from the rocks in the lower part of the valley. Blocks which fall into the *nevé* or granular snow high up, sink into it and disappear for a time; but it is curious, that except those which tumble into *crevasses* and reach the bottom, they all afterwards rise to the surface. Agassiz thinks, that the internal dilatation which makes the glacier travel downwards, also operates upwards, and carries all included masses to the surface. It is certain that an enclosed boulder is never seen in the terminal section of a glacier, where the composition of the mass can be best observed. In consequence also of the sides of the glacier travelling faster than the middle, and of its breadth generally diminishing towards its lower end, it very often happens that the blocks of medial moraines find their way to the sides and join the lateral ones.

The ascent of blocks from the middle or lower part of the ice to the surface, explains another curious fact—that though the general motion of the glacier is along an inclined plane downwards, scratches are often found on the rock inclined in the opposite direction; that is to say, supposing the surface of the glacier to dip at 10 degrees to the north, you will find scratches dipping at 10 or 20 degrees to the south, or even vertical. These are caused, in Agassiz's opinion, sometimes by inequalities in the bottom of the valley, but frequently by enclosed blocks working their way upward by the expansion of the ice, while the glacier is travelling downwards.

Figure 2 represents the usual form of a lateral moraine in the cross section, and as it would appear on a surface considerably inclined; *m* the mass of gravel forming the moraine; *r k* the rock on which it rests. Figure 3 represents the form which, it is assumed, a compact medial moraine would have if the ice were melted, and the matter left on the surface of the valley.

Fig. 2. Fig. 3.

Retreating glaciers form a terminal moraine every year, as mentioned in page 353; and in this case we might expect to find a series of mounds transverse to the valley, like *x y*, figure 5.

Fig. 4. Fig. 5.

Erratic Blocks.—Single blocks of huge size are often seen resting on the surface of the glacier, and travelling downwards with it. These are generally angular, and they often stand on pedestals of ice, as in figure 4, where *a* is a tabular mass of rock, and *b* the pedestal of ice.* Agassiz describes one he saw on a glacier, which measured 20 feet by 12, and must have weighed 100 tons or more. In accounting for the pedestal *b*, he observes that gravel, when it rests on the surface of a glacier, being heated through and through by the sun's rays, melts the ice below it, and gradually forms a pool or well in it. A large block, on the other hand, has only its upper surface heated, while the inferior mass, remaining cold, protects the ice below—both from the action of the sun's rays, and from the evaporation by which ice, like water, wastes away in the open air, and thus,

* This figure is borrowed from Agassiz's fourteenth plate. Figure three, and all the others, are ideal, and are suggested by his descriptions.

while small stones often sink into cavities, large ones seem hoisted on pedestals. Masses of all kinds tend towards the sides of the glacier, and many of these huge blocks are found scattered along the flanks of the Alpine valleys, some having remained there, stranded as it were. Others are found in the middle, far from existing ice, and were probably left there when the glacier disappeared. We have thus an explanation of the erratic blocks so common in this country, when these do not come from very distant stations. Being stranded by their greater weight, while the smaller matter moved onward, or left sticking on the soil in consequence of the final fusion of the ice, we can understand why they are often found *perched* on the sides of steep declivities.

Blocs perchés, so named for the reason just given, are sometimes found in very singular situations.

Let *a*, figure 6, be the surface of the glacier, *r* the top of a projecting rock *in situ*. The ice has the block *b* floating on it; it encompasses the fixed rock nearly on a level with its summit, and in travelling downward strands the block upon it. The block may be stranded on the very summit, as *c*. Supposing the glacier afterwards to disappear, here we would have an angular block perched on an isolated hill, or as Agassiz terms it, a pyramid, with a steep declivity below it, and we would be puzzled to conceive by what agent it was planted in so singular a situation.

Fig. 6. Fig. 7.

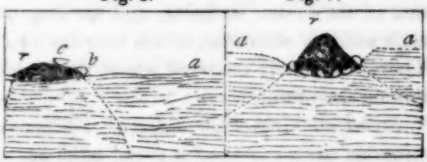

Figure 7 represents erratic blocks in a different situation, but quite as singular. *r* is a projecting fixed rock, rising considerably above the glacier *a a*; the reflection of the sun's heat from its surface melts a portion of the ice, and forms a cup-shaped cavity round it. Into this cavity blocks of various sizes fall by their weight from the surface of the ice as it glides onward, and settle on the flanks of the hillock. Supposing the glacier to disappear, this conical rock would have a ring of stones like a coronet encircling its summit, and we would be apt to wonder at the mysterious agency which brought them there, and left the lower parts of the hill destitute of them. Agassiz names various isolated rocks amidst the Alpine glaciers with such circles of stones round them, or with single blocks stuck upon them, as in figure 6. The same phenomena reappear on Mount Jura, where no glaciers now exist.

Creux and Lapiaz.—On the sides of the Swiss valleys, round holes, such as cascades make, are sometimes found in the rock; but in places remote from running waters, and where the form of the surface will

not permit us to suppose that any cascade could ever have existed. In other cases, a long, sinuous, dry, water-worn gutter or channel is observed, the course of which runs *across*, instead of along, the natural declivity of the ground. The study of the glaciers has enabled Agassiz to find a key to these enigmatical phenomena, which had perplexed previous inquirers. Streams of water flow along the surface of a glacier, and when one of these falls into a fissure which is open to the bottom, it often forms a cascade, and cuts a round cavity in the rock with the gravel and sand which it either finds there, or carries down with it, as some of our rivulets work out the hollows termed cauldrons. When no fissure exists, the stream sometimes cuts a funnel or shaft (*couloir*, *entonnoir*) through the ice by the action of gravel. If the glacier is travelling downwards, the cascade will travel with it, and convert the round cavity in the rock into a long gutter; or, supposing the water to reach the bottom without falling in a cascade, still, in finding an issue below the glacier, it will be compelled to follow the sinuous openings left by inequalities in the bottom of the ice, and thus take a course at variance with the natural inclination of the surface. We have here an explanation of the *creux*, or holes, and the long water-worn gutters found in such unlikely situations, which bear the local names of *lapiaz* or *karren*. These are chiefly observed where the rock is soft, and are seldom visible on the granite.

Stratified Gravel on sides of Valleys.—When a small portion of stratified gravel or sand is found adhering to the side of a valley, high above its bottom, the conclusion usually come to is, that a lake or arm of the sea had once filled the whole up to that level, and that the deposit is merely a remnant of one much more extensive. Agassiz has shown that this conclusion may be erroneous. When the streamlets flowing on or under a glacier, cannot find an escape below, they often form small lakes at the surface on its flanks, and, as in other lakes, the gravel and sand carried into these, arrange themselves in strata. This stratified deposit may be continuous with, and form as it were a portion of, a lateral moraine, which is not only unstratified, but which follows a line probably far from level. Here again the study of existing glaciers enables us to explain very anomalous appearances.

Glacier Barriers.—A glacier descending a valley opening into another, sometimes pushes forward till it forms a dike or barrier across the latter. Behind this the water collects and constitutes a lake, which augments till it breaks the icy barrier, or flows over it, producing frightful inundations. In 1815 the glacier of Getroz formed a dike across the valley of Bagnes. This dike went on increasing till 1818, when it was 500 feet high and 800 long. It was then burst by the pressure of the waters of the Drance, which committed terrible ravages as far

down as Martigny. The lake of Distel, on the Saas, those of Rufnen and Gurglen, in the Tyrol, and that of Passey, on the Adige, are also formed by glaciers. The last has burst its dike six times, with the most destructive effects, since 1404. Agassiz traced decided marks of an ancient glacier at the north side of Ben Nevis. This glacier, he thinks, had closed up the valley of the Spean, and formed a lake in Glenroy, in which the banks of gravel, called Parallel Roads, were deposited. The barrier being of ice, which subsequently melted, the absence of any marks of its existence is accounted for. At present we shall not stop to inquire whether this theory or Mr. Darwin's is the more probable.

Alluvial Deposits.—Agassiz thinks that the floods produced by the bursting of such lakes as those described, and by the fusion of the ice, tore up the moraines, scattered their materials over the country, and formed the unstratified boulder clay, and the stratified sand and gravel resting upon it, which now cover nearly the whole surface of the low country.

Ancient extent of Glaciers in Switzerland.—The traces of ancient lateral moraines are seldom very distinct; yet in the lower valleys, where no glaciers now exist, in that of the Rhone, for instance, between Martigny and the lake of Geneva, several may be seen ranged in parallel lines, one above another, at 1000, 1200, and even 1500 feet above the river. Terminal moraines are found half a mile, a mile, a league, and even several leagues from existing glaciers; but these are in secondary valleys, and belong to the period when the glaciers were retreating into the narrow limits which they now occupy, while the floods which occurred at this period had obliterated those of the principal valleys. The striated and polished surfaces, which had a more durable existence, are found at great heights; among other examples, on Seideihorn, (an isolated mountain in the Alps, now destitute of glaciers,) 2590 feet above the bottom of the valley, indicating that ancient glaciers of this depth or more existed here. The boulders also, or *blocs perchés*, the *creux* or pits, and the *lapiaz* or water-worn gutters, were all observed far beyond the present limits of the glaciers. This first step in the argument conducts Agassiz to the conclusion that *the whole of the Alps, at some ancient period, formed one vast mer de glace*, the ice descending to the level of the great Swiss valley which separates these mountains from Jura.

But the same indications of glacial action exist on Mount Jura, which runs parallel to the Alps, divided from them by the great Swiss valley, fifty miles in breadth. This chain, which is of moderate height, is now entirely destitute of glaciers, and, owing to the nature of the rock, the marks of abrasion are remarkably numerous and distinct. They are found *on the side fronting the Alps* from the bottom to the summit, and

from Ecluse, near Geneva, to Aarau, a distance of 130 miles. (*Sur tout le versant meridional de Jura*, depuis le Fort de l'Ecluse jusqu'aux environs d'Aarau.) When the surface is newly exposed, it is smooth as a mirror, marked with furrows and fine scratches, and exhibits the *roches moutonnées*, or rounded undulations and domes. But the most characteristic fact is, that *the furrows do not run from the summit downward, but in a horizontal or oblique direction, along the face of the ridge*, showing that they were impressed by a body moving parallel to the chain along its southern flank. In form and position, they are, in short, precisely similar to the furrows produced by existing glaciers on the sides of the valleys *along* which they move. Further, these polished and striated rocks are not confined to the declivities of Jura, but are found equally at their foot, in the bottom of the great Swiss valley, wherever the rock is calcareous.*

In addition to these striated and polished surfaces, Jura has its moraines, and in these moraines patches of stratified deposits are found, such as are now formed in small lakes on the flanks of glaciers. It has thousands of erratic blocks, distinctly derived from the Alps; and, that nothing might be wanting to complete the chain of evidence, Jura has its *lapiaz*, or water-worn gutters, where no water now runs; its *creux*, or water-worn pits, in situations not dominated by any rock whence a cascade could fall; and its salient peaks, surrounded by coronets of boulders, as in figure 7. Now, as no ridge occurs between the Alps and Jura, it is evident that the mass of ice which pressed against the southern declivities of the latter to the height of 3500 feet or more, with a force sufficient to cut and groove the surface longitudinally, must have extended far into the great valley or low country; and as striated rocks and travelled boulders are also found all over the bottom of that valley, and on the Alps at its opposite side, we have before us a concatenated series of facts, leading almost inevitably to the conclusion that a *mer de glace*, or vast sheet of ice, once enveloped the Alps and Mount Jura, and covered the whole of the low country between them. Hemmed in by the two mountain chains, the ice could expand only in a northeast or southwest direction, and Agassiz infers from the direction of the *striæ*, that in the middle and northern part of the valley the motion was northeastwards, or towards the lake of Constance.

Erratic Blocks of the Alps and Jura.—The large Alpine boulders found on Mount Jura, forty or fifty miles from their native rock, have been a stumbling block to geologists for the last half century. As the subject, though often discussed in books of science, may be new to some individuals, we shall premise a short account of the phenomena.

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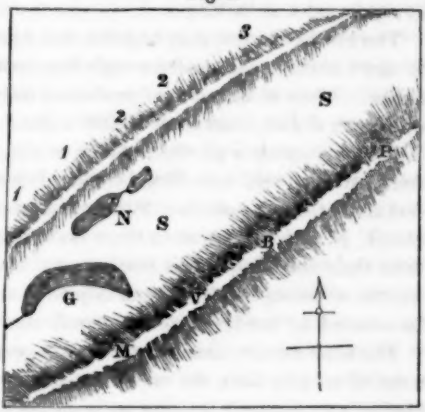
* Etudes sur les Glaciers, p. 291.

Mount Jura rises at some points to the height of 5,000 feet* above the sea, and 3,500 above the great valley of Switzerland on its south side. The Alps run parallel to Jura at the distance of fifty miles, and their higher summits have an elevation varying from 11,000 to 15,000 feet above the sea; but the northern skirts of the chain are a great deal lower, and their distance from Jura scarcely exceeds thirty miles. The following diagram will convey an idea of their relative position:

Fig. 8.



M V B P, The chain of the Alps extending north-east and south-west.

1 1, 2 2, 3, The chain of Jura, running parallel to the Alps.

S S, The great valley of Switzerland separating the two chains.

G, The lake of Geneva; N, the lake of Neuchatel.

The Alps consist of primary rocks, granite, gneiss, &c., in the centre, flanked by secondary. Jura consists of different formations of limestone, all belonging to the oolitic series.

The two chains, in distance, bearing, and position, may be compared to the Ochil and Lammermuir hills. If we suppose the Ochils to be *twice*, and the Lammermuirs *six times* as high as they are, and the valley between them, constituting the basin of the Forth, to be three or four times as deep as it is, we shall have a pretty good idea of the physical features of the district under consideration.

Now the fact which has so long exercised the ingenuity of geologists is this. Hundreds of huge fragments of primary rocks, distinctly recognizable as portions of the Alps, are found perched on the southern declivities, or resting in the interior valleys of Jura, forty or fifty miles from their native locality; and geologists have been perplexed to discover by what agency these erratic blocks have been transported across the great Swiss valley, and placed in the singular situations where we find them. The magnitude, external appearance, and distribution of these masses, present circumstances worthy of notice.

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* The measures are always in French feet, which may be converted into English by adding one fifteenth.

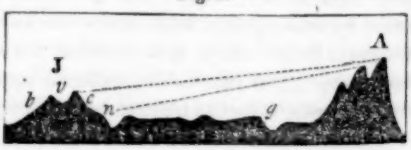
Von Buch, Escher, and Studer, have shown, from an examination of the mineral composition of the boulders, that those on Western Jura, 1 1, have come from the region of Mont Blanc, M, and the Valais, V; those on the middle parts of Jura, 2 2, from the Bernese Oberland, B; and those on Eastern Jura, 3, towards Aargau and Zurich, from the Alps of the Petits Cantons, P. The blocks have thus been derived from the parts of the Alps nearest, generally speaking, to the localities where we now find them, as if they had passed across the valley in a direction at right angles to its length.

The blocks are generally angular, and therefore had not been exposed to much attrition, either from agitation amidst gravel, or from mutual action. Many of them are of prodigious magnitude. The famous mass of *Pierre à Bot*, containing 50,000 cubic feet, and weighing probably 4,000 tons, equals a goodly mansion in size, namely, one of 30 feet in front, 40 in depth, and 40 in height. It rests on a part of Jura 2,177 feet above the sea, and about 900 feet above the level of the lake of Neuchatel, N. Near Chaumont there is a group of granite blocks, which, from their magnitude, their number, and their juxtaposition, look like a hamlet of cottages. The large Alpine boulders of Jura, in short, may be counted by hundreds, and the small ones by thousands.

The boulders are distributed in zones on the terraces, which, like the steps of a stair, form the out-goings of the different formations. The highest are disposed in rings, as in figure 7, round the lower summits of Jura, at a height between 3,000 and 3,300 feet above the sea. The other zones occur on the terraces below this; the first at elevations from 1,900 to 2,400 feet; the next at 1,600 to 1,800 feet; and the last descends to the level of the lake of Neuchatel, 1,324 feet above the sea. Moreover, these travelled blocks penetrate into the transverse and into the interior valleys of Jura, and some are even found at the back of the chain, near the Doubs.*

Saussure attributed the transportation of these boulders to a *debacle*, or great current, rushing from the Alps; and Von Buch, finding that one current would not account for the phenomena, assumed the existence of several. But the inadequacy of such explanations is obvious.

Fig. 9.



A, The Alps; J, Mount Jura, with the great valley, fifty miles wide, between them.

e, The southern declivities of Jura, upon which most of the erratic blocks rest.

* Etudes, p. 278-280.

v, The interior valleys, and *b*, the back of the chain, where some of them are found.

n, The lake of Neuchatel.

g, The lake of Geneva.

The difference of altitude between the Alps and Jura, distributed over a space of fifty miles, gives an inclination of no more than two degrees. Now, no current could force, or rather float, masses of stone, weighing 1,000 tons, across an uneven valley of such breadth, although the difference of level were much greater. Even if the valley had then been filled up with gravel, or other solid materials, and formed a regular inclined plane, as Ebel and Dolomieu assumed, the blocks could not have been moved over it by water; or, if moved, they would have been rounded by attrition; and, instead of being disposed in zones, they would have been accumulated *pel mel* at the bottom of Jura. It must be kept in mind, that the erratic blocks are found on the Italian side of the Alps as well as the Swiss, and that currents and inclined planes would be required in both directions.

A more recent hypothesis, which assumes that the boulders were transported by icebergs when the great Swiss valley was under the sea, is much more plausible. Agassiz objects to it, that it does not account for the coat of sand and gravel covering the sides of the mountain on which the large blocks generally rest, nor for the striated, grooved, and polished surfaces, nor (he might have added) for the *lapiaz* and *creux*, and the lateral moraines which deviate from a horizontal position.

It will be anticipated that Agassiz transports the boulders across the great valley on a bridge of ice. He observes that the eastern Alps, as they have disturbed the *diluvium* containing bones of elephants, must have been raised up since that deposit was formed, and their upheaval is the last *cataclysm*, or geological convulsion, which has visited Europe. Previous to this event, an immense mass of ice had covered the surface of the northern parts of the old and new world; "but when the upheaval of the Alps took place, this formation of ice was raised up like the other rocks; that the fragments detached from the fissures of upheaval (*fentes du soulevement*) fell upon its surface, and without being rounded—since they were not exposed to friction—moved along the inclined surface of the sheet of ice, in the same manner as the fragments of rock which fall upon glaciers are carried to their sides in consequence of the continual movement produced in the ice by its alternate thawing and congelation, at the different hours of the day, and the different seasons.

"After the upheaval of the Alps, the earth must have recovered a higher temperature; the ice in melting produced large funnels (*entonnoirs*) at the places where it was thinnest; valleys of erosion were ex-

cavated at the bottom of these openings, in localities where no current could exist unless enclosed between walls of ice; and, when the ice disappeared, the large angular blocks were found resting on a bed of rounded pebbles, of which the smallest, often passing into a fine sand, form the base."

The description of the supposed phenomena attending the upheaval of the Alps, though it forms the very kernel of his theory, is less clear than the other parts of M. Agassiz's work, which is generally very perspicuous; and instead, therefore, of giving the substance of his statements in our own language, we have translated the two most important passages literally. In a paper read before the Helvetic Society of Natural History in 1837, containing the germs of the theory, more fully unfolded in his new work, he thus expresses himself:—

"The appearance of the Alps, the result of the greatest convulsion which has modified the surface of our globe, found its surface covered with ice, at least from the North Pole to the shores of the Mediterranean and Caspian Seas. This upheaving, by raising, breaking, and cleaving in a thousand ways, the rocks which compose the prodigious mass that now forms the Alps, at the same time necessarily raised the ice which covered them; and the debris detached from so many deep upbreakings and ruptures, naturally spreading themselves over the inclined surface of the mass of ice which had been supported by them, *slid along the declivity to the spots where they were arrested*, without being worn or rounded, since they experienced no friction against each other, and, even when arrested, came in contact with a surface so smooth; or, after being stopped, they were conveyed to the margin or to the clefts of this immense sheet of ice, by that action and those movements which characterize congealed water when it is subjected to changes of temperature, in the same manner as the blocks of rock which fall upon glaciers, approach their edges in consequence of the continual movements which the ice experiences, in alternately melting and congealing at the different hours of the day and seasons of the year."—*Edinburgh New Philosophical Journal, No.* 48, *p.* 378.

The words in italics indicate an opinion that some of the boulders might have *slid* from the Alps to Jura on the surface of the ice, while others adhered to it, and only travelled as the angular blocks resting on glaciers now travel. Nothing equivalent to these words occurs in the *Etudes*, and even the distribution of the fragments by the more tardy process is not very clearly explained. We are not sure, for instance, whether he means that the ancient *mer de glace* rose above Jura, and determined the progressive motion of the ice in a direction away from the Alpine chain at right angles, bearing the boulders first detached over Jura into the basin of the Doubs, and that, owing to the

gradual fusion and subsidence of the ice, the later boulders were stopped in their motion by that mountain and settled on its southern declivities. He seems, however, we rather think, to mean, that the glaciers of the Aar, the Kander, and the Rhone, were lateral and auxiliary to that of the great valley; that the dilatation of the ice (and the motion of the boulders) following the course of the troughs in which it lay, was northwest in the lateral valleys, and northeast or southwest in the great valley; and that the blocks resting on Jura are to be considered as stranded on one side of the great glacier, the motion of the eastern portion of it being northeast, while that of the western was southwest. We see some objections to this conclusion. The transference of blocks from B, for instance, (figure 8,) should not have been right across to 2-2, but diagonally to 3, or 1-1, according as the expansive motion of the ice was northeast or southwest. A theory, however, which explains so many facts, is not to be rejected on account of minor difficulties, which future researches may clear up.

When the *mer de glace* was melting, the first openings through it would be formed where it was thinnest. The water engulfed in these would seek out channels where the fissures or vaults under the ice left room for it, and *valleys of erosion* would thus be excavated, sometimes at variance with the natural declivity of the ground, and which would afterwards become the channels of rivers. Such valleys do occur, and the explanation is simple and probable. But account should have been taken of the heat developed along the fissure of upheaval, which would produce floods of water at the most elevated points; for when the granite ascended from below, though it was in a solid state, it must have brought with it the temperature of the region from which it came. The heat thus generated must have been increased by the enormous friction on the pre-existing primary strata, when they were fractured and bent up; and the ice in contact with these strata, which surrounded the highest summits of the Alps, must have been first melted. Here was an obvious source of formidable *debacles*, which must have produced great changes on the surface of the adjacent countries.

As portions of the old alluvium, containing bones of the fossil elephant, have been found turned up on the flanks of the Alps, Agassiz infers that deposits of clay and gravel existed before the icy envelope was formed; that these must have been broken up and remodelled by the streams arising from the fusion of the ice; and, consequently, that part of the existing alluvial cover is derived from the wrecks of one more ancient.

When the ice retired from the great valley or low country, into the lateral valleys of the Rhone, the Rhine, the Aar, and others, the formation of moraines would begin; and the clay, sand, and gravel thus collected at particular localities would be dispersed and remodelled by

the bursting of glacier lakes, occasionally formed in the upper parts of valleys by barriers of ice. Hence the origin of a second portion of the existing alluvial cover.

The deposits of clay and gravel spread over the great Swiss valley, must be due to floods arising from both the causes just mentioned. These floods, Agassiz thinks, must have had a depth of not less than 300 feet, for the sand and fine gravel found on the higher parts of Jura have been washed off from the lower to this height. Masses of ice, forming icebergs, would occasionally float in them, and carry boulders from one place to another.

Sheets of ice occupied the lakes of Geneva, Neuchatel, and others, at this time, and prevented them from being filled up by the dispersion of the alluvial matter.

The clay containing the bones of fossil elephants on the sides of the Alps, he considers as contemporaneous with the deposits entombing similar remains on the northern shores of Siberia, and he infers that one and the same catastrophe had enveloped these districts, and all the northern parts of both continents, in ice. The catastrophe had arrived suddenly; for, as Cuvier remarks, the Siberian fossils show by their numbers that the animals had lived where their remains are found, and by the actual preservation of the flesh and skin in some cases, that they had rested but a short time on the ground before the ice covered them. The retreat of the ice, however, had been slow, as demonstrated by the moraines forming a series in some valleys, with a gradually decreasing range, both in extent and elevation. The present glaciers may be considered as the puny and feeble representatives of that vast crust of ice which formerly enveloped the northern parts of the globe.

The great incrustment of ice necessarily extinguished organic life, so far as its domain extended. The animal tribes which then perished —the mastodon, Elephas primigenius, rhinoceros, and others,—have left their remains in the alluvium, and are found closely to resemble the existing races, which were of course introduced after the ice disappeared, and the region acquired the temperature necessary for their support.

Agassiz thinks that a similar great and sudden depression of temperature probably served the same purpose at earlier periods, by clearing the globe of one zoological group, to make room for another.

Mountains, of whose rocks fragments are found transported to a distance, in different directions, are considered as *centres of dispersion*, by Agassiz. Thus, the Alps, whose boulders strew the plains of Switzerland, Italy, Austria, and France, form one centre of dispersion, embracing Jura within its range. The Vosges (in Alsace), which exhibit the same phenomena on a smaller scale, are another. The Ce-

vennes are probably a third; and the Pyrenees a fourth. We have one of vast magnitude in the Scandinavian mountains, whose travelled blocks are found scattered over northern Europe, from the shores of England to Moscow. In this country Agassiz considers the Grampians, the Cumberland mountains, and those of Wales, as centres of dispersion.

There is a question arising out the theory, which he has not touched upon. If we suppose the region from the 35th parallel to the north pole to be invested with a coat of ice thick enough to reach the summits of Jura, that is, about 5000 French feet, or one English mile in height, it is evident that the abstraction of such a quantity of water from the ocean would materially affect its depth. The area of the space extending 55 degrees on each side of the pole, is pretty correctly *two-sevenths* of the whole surface of the globe. Supposing two-thirds of this space to be dry land, and the spongy coat of ice equal to two-thirds of its bulk of water, and assuming, what is pretty near the truth, that the sea occupies three-fourths of the surface of the globe, we find that the abstraction of the water necessary to form the said coat of ice, would depress the ocean about 800 feet. Admitting further, that one-eighth of the fluid yet remains locked up in the existing polar ices, it follows that the dissolution of the portion which has disappeared would raise the ocean nearly 700 feet. The only very uncertain element here is the depth of the ice; but even if this should be reduced one-half, we would still have an agent capable of producing a change of 350 feet on the level of the sea. We are besides leaving out of view the southern polar region, which it is now known embraces a great extent of land. If this was also covered with ice, the change would be much greater than we have assumed.

These very original and ingenious speculations of Professor Agassiz must be held for the present to be under trial. They have been deduced from a limited number of facts observed by himself and others, and skilfully generalized; but they cannot be considered as fully established till they have been brought to the test of observation in distant parts of the world, and under a great variety of circumstances. Supposing the theory to be substantially sound, the magnitude of the consequences it involves will undoubtedly bring objections to light, which may render modifications necessary, both in its principles and its details. In the mean time, it assists us in resolving some difficulties. It contributes, in a greater or less extent, to explain the dispersion of erratic blocks, the *bizarre* situations they occasionally occupy, the banks of clay and gravel found on the sides and at the mouths of valleys, the *striæ*, polishing and grooving, observed on the surface of rocks *in situ*, and of large stones in the till; and it promises to throw light on what is at present a very obscure subject, the origin of the older and newer alluvium.

ART. XVII.—*On a New Species of Trilobite of very large size;* by JOHN LOCKE, M. D., Prof. of Chem. and Pharm. in the Medical College of Ohio.* Communicated to this Journal by the author.

Isotelus megistos.

Clypeo, antice elliptico attenuate marginato postice arcuato, et terminato utrinque aculio; cauda postice elliptica, antice arcuata; articulus abdominis octo.

The shield is anteriorly nearly perfectly elliptical, broadly and thinly margined, posteriorly arcuate, and terminated at the angles by spines or pointed processes extending backwards beyond the two first abdominal articulations. The eyes are prominent, large, furnished exteriorly each with a crescent-shaped cornea, and placed rather nearer to the posterior edge than to the outer margin of the shield. From the corner of each eye a sutural line extends forward, meeting at the anterior margin of the shield, and enclosing a lozenge-shaped, leaf-like frontal space. Abdomen trilobited; middle lobe cylindrical; articulations eight, bending flatly over the middle lobe, and descending abruptly at their lateral extremities, which are broad, flat, and rounded beneath, and admirably fitted to sliding over each other when the animal should contract or roll himself, according to a well known habit of the genus. Tail posteriorly elliptical, anteriorly circularly arcuate, length measured horizontally, less than two thirds of the width, having two obscure longitudinal depressions continuous with the abdominal furrows, and converging towards an obscure posterior tubercle. The anterior outline of the tail exhibits three slight lobes, (corresponding with those of the abdomen,) the two exterior of which are very distinctly marked by a transverse depression.

When the posterior shell of the tail is decorticated an interior shell is exposed, which forms all round a deep trough or "cavetto," beautifully marked with a "venalian" of eccentric curved and branched lines. The above named posterior tubercle is very nearly the "focus" of the "elliptic" outline of the tail, is just anterior to the marginal cavetto, and is the centre around which the curved lines originate, each passing a little further back than the other and advancing outwardly and forward until they successively disappear on the anterior margin of the "cavetto."

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* Read before the Association of American Geologists at Philadelphia, April 6, 1841.

Distinctions.—This Isotelus resembles the *gigas*, from which, however, besides the aculeate processes, it is distinguished by the perfectly elliptic terminations, by the simple (not raised) margin of the shield, and by the proportions of the tail, the *gigas* having the length $\frac{1}{3}$ths, and the *megistos* $\frac{2}{3}$ths only of the width. The latter is also much more prominent than the former, and the tail and sides much more abrupt in their descent. From the *megalops* and the *stegops* it is clearly distinguished by the eyes.

History and mathematical proportions.—The first fragment (see outline on Plate III) was discovered by myself in Adams county, Ohio, in 1838. It was about six inches of the marginal "cavetto" of the tail, beautifully veined, marked with the tubercle, perfectly elliptical, and coinciding with the end of an ellipse twenty two inches long and twelve inches broad. The second specimen was an entire tail found at the same locality; this, upon admeasurement, was found to coincide with an ellipse of exactly half of the dimensions of that which suited the first specimen, and showed, by a fortunate fracture, the internal marginal cavetto. These two specimens were both figured and described by me in the Ohio geological report for 1839.

The third specimen (see outline) was discovered in autumn of the same year by Wm. Burnett, Esq. on the hills at Cincinnati, and presented to me soon after. It was partly covered by the crystalline blue limestone in which it had been imbedded, and it was not until the winter of 1840–41 that I dissected it out of its gangue, and found that it had an aculeate shield, and that it exhibited the animal almost entire.

It is of the same dimensions as the second specimen, and measures nine inches and three fourths in length, and six inches in breadth. The first fragment must therefore have been from a specimen nineteen inches and a half long, and twelve inches broad. These gigantic dimensions suggested the name *maximus*, which I gave in the Ohio report, but which, for obvious reasons, I have changed to the more classical Greek term of the same import.

The fourth specimen was discovered by Mr. Carley, of Cincinnati, who was the first to discover the aculeate shield, for in the Burnett specimen this character was still concealed. Mr. Carley's specimen appears to be a young one, for it is only about three inches long. It was obtained in the bed of the Ohio river about four or five hundred feet lower than the situation which furnished the Burnett specimen. My own first specimens were found within thirty feet of the top of the blue limestone formation, where it is overlaid by the cliff limestone. Now the character of this magnificent species of trilobite has been ascertained, it is evident that fragments of it are abundant in our blue limestone, which is undoubtedly the equivalent of the limestone of Trenton

Falls, N. Y., called the Trenton limestone. The most common fragment found is the corner of the shield with its thorn-like appendage, (see the figure, Plate III.) For the information of geologists, I would observe, that figure 2 was found just below the stratum most abundant in the genera Delthyris, Turritella, and Trochus, and that Mr. Carley's specimen occurred in the region of the Isotelus gigas, and the Cryptolithus tesselatus.

ART. XVIII.—*Register of the Thermometer from* 1830 *to* 1839, *kept at Boston, Mass.;* by J. P. HALL.

Years.	Jan.	Feb.	Mar.	Aprl.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean of year.
1830	25.73	25.94	37.55	48.07	57.08	66.67	72.04	69.95	59.27	52.70	46.68	35.22	49.74
1831	23.38	24.86	41.34	48.51	59.31	71.47	73.09	72.39	63.08	53.76	40.82	19.14	49.26
1832	27.38	28.73	36.98	41.99	53.15	64.48	68.04	69.85	61.24	52.15	41.78	31.35	48.09
1833	31.16	25.45	33.03	48.74	58.91	63.23	72.06	67.04	62.28	50.56	38.30	31.84	48.55
1834	24.85	34.23	37.53	46.88	53.44	63.84	74.03	68.28	62.86	49.03	38.52	28.55	48.50
1835	27.26	25.46	32.80	43.84	55.26	65.42	71.75	68.75	57.90	53.54	40.22	23.43	47.14
1836	26.94	20.95	31.84	43.73	55.44	58.91	69.19	65.12	60.46	45.22	36.77	29.52	45.34
1837	21.74	25.70	31.86	44.72	52.97	63.75	68.45	65.32	59.00	48.03	39.26	28.93	45.81
1838	33.28	19.27	35.79	41.66	54.84	68.86	74.22	69.14	61.03	47.70	35.65	26.72	47.35
1839	27.12	29.18	35.71	47.48	56.45	62.54	72.49	68.93	62.20	51.39	37.62	31.90	48.58
Mean of 10 years,	26.88	25.98	35.44	45.56	55.68	64.91	71.54	68.48	60.93	50.40	39.56	28.66	47.83
Mean of th' 10 y'rs f'm 1820 to '29,	26.23	29 57	36.14	46.13	57.57	67.00	72.41	69.89	62.60	51.35	39.87	32.38	49.26

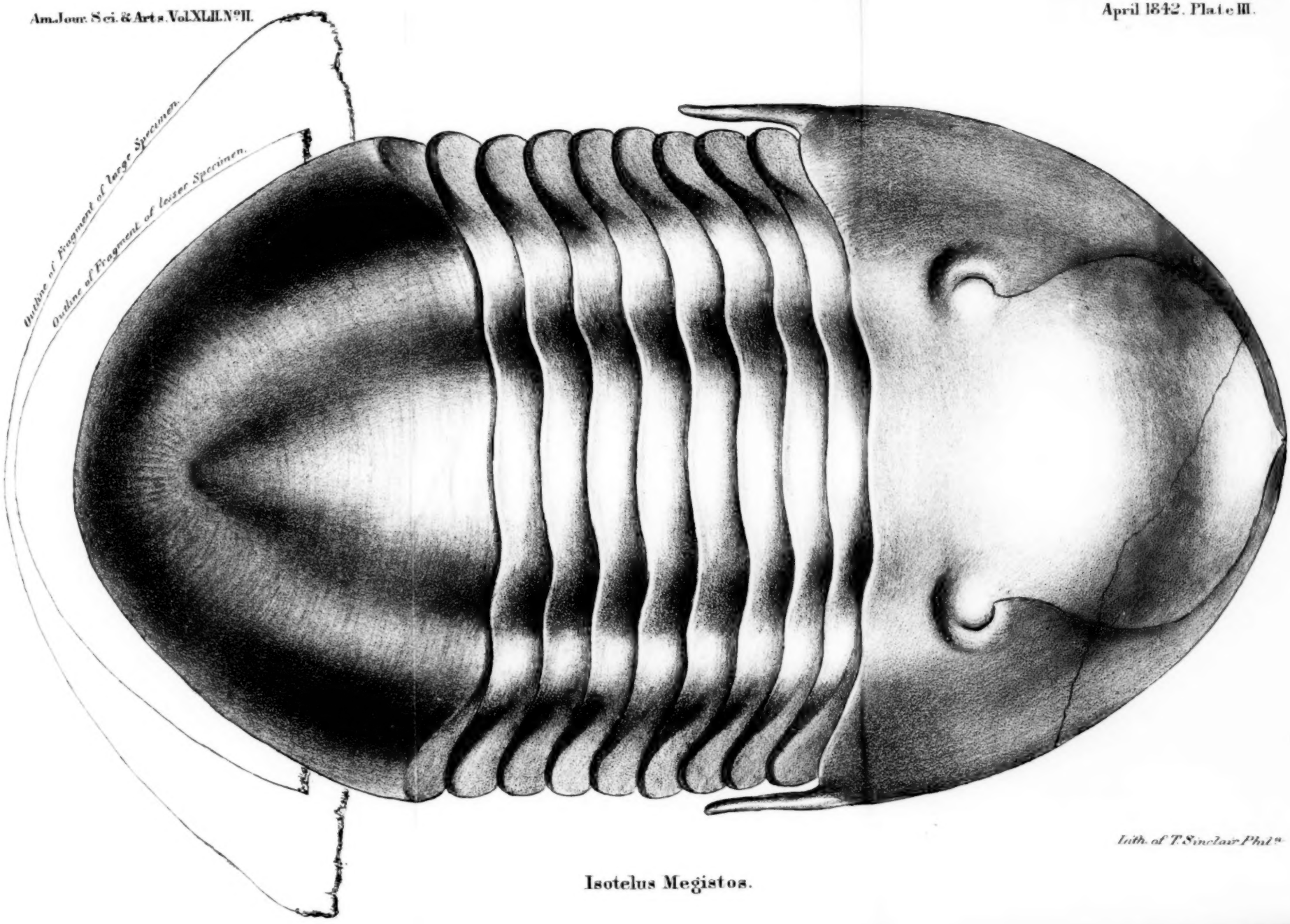
From the table it will be seen, that the mean temperature of every month except January, was lower in the ten years from 1830 to 1839, than in the ten years from 1820 to 1829.

Two years (1836 and 1837) were of remarkable coldness. In these years, the crops of grain and corn were cut off to such an extent, that large importations were necessary to supply the demand.

The thermometer rose to 99° on the 21st and 22d of July, 1830, and the 26th of July, 1834, and fell to 10° below zero on the 4th of January, 1835, and 24th of January, 1839.

Hours of observation, 7, A. M., 2 and 9, P. M.

Boston, Mass., 1841.



Isotelus Megistos.

Lith. of T. Sinclair Phil.ª

ART. XIX.—*Chemical examination of Bituminous Coal from the pits of the Mid Lothian Coal Mining Company, south side of James River, fourteen miles from Richmond, Virginia, in Chesterfield County;* by B. SILLIMAN, Professor of Chemistry, &c. in Yale College, and O. P. HUBBARD, Professor of Chemistry, &c. in Dartmouth College.

THREE specimens of fair average quality, not selected for any apparent superiority, were taken from a hogshead of the coal, sent by the President of the company, A. S. WOOLDRIDGE, Esq., and experiments were made upon portions of these samples indiscriminately taken.

Physical Characters.—The coal is in the fresh fractured surface of a jet black color; lustre, resinous and splendent; fracture, slightly conchoidal; splits easily, parallel to surfaces of deposition which are strongly marked; the two sets of slines considerably distinct in large masses and in small specimens very distinct, showing a rhombic structure, in several specimens before us, making with each other angles of 78° and 102°.

There is another series of faces, very lustrous and splendent, that also intersect at angles of 78° and 102°. These two series of faces cross each other and the surfaces of deposition, and give rise to two rhombohedra that incline in opposite directions. By these the coal is intersected so frequently as to divide it into layers of a line in thickness in one direction. The coal is compact, and the specific gravity of three samples taken as above, was

A. 1.281
B. 1.312
C. 1.284

3.877÷3=1.292. Sp. gr. water being 1.

No. 1. Sixty three and a half grains were coked for two and a half hours, in an iron bottle in a draft furnace, and the gaseous products were collected dry over mercury.

a. All the jars of gas, eighteen in number, were examined by caustic potassa; the carbonic acid was thus absorbed, and was equal to 80 cubic inches, or 1600 parts, being two fifth parts of the volume of the gas.

b. Binoxide of nitrogen gave in jar 1, a slight redness, thus indicating oxygen gas.

c. Acetate of lead added gave no indication of sulphuretted hydrogen.

d. Sulphate of copper gave no indication of ammonia.

e. The gas remaining in jar 1, after removal of carbonic acid gas, having been generated at the lowest temperature, burned with the clear, dense, yellow flame of *olefiant gas.*

f. The gas remaining in the other jars, after the removal of the carbonic acid gas, was entirely combustible, and burned with a flame resembling that of a mixture of carbonic oxide and light carburetted hydrogen.

g. The volume of all the gases of *f* and *g* was equal to 120 cubic inches, or 2425 parts.

Ratio of carbonic acid, 80 cubic inches, 1600 parts : 2
" combustible gases, 120 do. do. 2425 " : 3

h. The coke of No. 1, was very light, jet black, shining and soft, and was not estimated because it was mixed with portions of iron from the interior of the bottle.

The following samples were coked for two hours in a draft furnace, in covered Hessian crucibles. The coke was harder than in the process in the iron bottle. Its color was jet black internally, and gray at the top of the mass, where probably the air had slight access, and was about twice the bulk of the coal employed.

The coke of 2 and 5 was burned in a platina capsule over a spirit argand lamp, till the carbon was all consumed. The results are as follows in the table, reduced to centesimal proportions.

No. 2. 53.5 grs. coal gave - - - - 33.8 coke.
3. 63.5 " " - - - - 39.6 "
4. 100. " " - - - - 67.6 "
5. 200. grs. coal gave 128.45 grs. which for $\frac{1}{2}$ = 64.2 "

	2	3	4	5	Sum.	Average.
Carbon,	54.76	62.36	67.6	56.1	4 110.86	55.43
Ashes,	8.41			8.1	2 16.51	8.25
Volatile matter,	36.82	37.63	32.4	35.8	4 142.65	35.66
	99.99	99.99	100.	100.		99.34
Coke, per cent.	63.17	62.36	67.6	64.2	4 257.33	64.33

Three specimens, Nos. 6, 7, and 8, (taken as heretofore,) of 50 grains each, were coked in close covered platina crucibles over an alcoholic lamp, and then removed to and ignited in a draft furnace at a white heat. The coke from all these was jet black, shining, porous, and soft.

The carbon being burned off in a platina capsule, the results were as follows, reduced to centesimal proportions.

	6	7	8	Average.
Carbon, . .	63.4	60.8	59.2	61.1
Ashes, . . .	4.	7.1	10.4	7.1
Volatile matter, .	32.6	32.	30.4	31.6
	100.	99.9	100.	99.8
Coke, per cent. .	67.4	68.	69.6	68.2

The average of both series is given below.

	First series.	Second series	Average.
Carbon, . .	55.43	61.1	58.26
Ashes, . .	8.25	7.1	7.67
Volatile matter, .	35.66	31.6	33.62
	99.34	99.8	99.55
Coke, . . .	64.33	68.2	66.31

Two specimens of 100 grains each, were heated in fine powder to 300°, and sustained a loss of 1.9 grains, and 2.1 grains; average loss 2 per cent.; this was moisture, which is of course included in the per centage of "volatile matter."

No bitumen or liquid matter was distilled over in the coking of No. 1.

The ashes in every case were very light, and of a clear grayish white, indicating no pyrites or peroxide of iron, and were in no degree attracted by the magnet.

The ashes of No. 7, being 3.6 grains, were treated with dilute nitric acid; 2.4 grains were insoluble residuum, chiefly silica; and the soluble matter was lime and alumina, slightly colored by oxide of iron and manganese.

The analysis of the coal, shows in the general average, (which may be regarded as approaching nearly to practical results, where the coal is employed in the large way,) proportions of solid carbon and volatile matters, which render it well adapted to the most important purposes in the arts, and probably, with a low and well managed heat, to the production of gas for illumination. Its

ashes are of a kind and in such a state as to offer no inconvenience in using it as a fuel. The coke also is in excellent form for producing intense heat.

To show more particularly the resemblances of this coal, we cite below, a collection of the analyses of various coals, some *dry* and others *fat* coal, from a Report on the Manufacture of Iron, made to the Legislature of Maryland, by J. H. Alexander, Esq.

	Frostburg. Maryland. *	Scotland. Clyde. *	Staffordshire. Tipton. *	Newcastle. †	Newcastle. †	Staffordshire. †	Rive de Gier. †	Cannel. Lancashire. *	Mid Lothian, Va.	Mid Lothian, Va. Averages.
Carbon,	66.3	64.4	67.5	60.5	67.5	62.4	66.5	64.72	61.1	58.26
Ashes,	14.3	4.6	2.5	4.	2.5	3.5	2.		7.1	7.67
Volatile matt'r	19.4	31.	30.	35.5	30.	34.1	31.5	35.28	31.6	33.62
	100.	100.	100.	100.	100.	100.	100.	100.	99.8	99.55

* Dry coals. † Fat coals.

The Newcastle coal, analyzed by Karsten, gave 68.5 per cent. of coke. The average of our results, by the last series of experiments, is 68.2 per cent. of coke.

The following table will show the relations of the Mid Lothian coal in its amount of coke.

Clyde,	69.	Rive de Gier,	68.5
Tipton,	70.	Lancashire,	64.72 Cannel.
Newcastle,	64.5	Mid Lothian,	64.33 aver. 1st series.
"	70.	"	68.2 aver. 2d do.
"	68.5	"	65.9 general average.
Staffordshire,	65.9		

The average of the three experiments upon the Newcastle coal, gives 67.6.

It appears that the Mid Lothian coal of Virginia, is substantially the same as the best coals of both Europe and America, while it is almost identical with the Newcastle coal of England.

Its proportion of excellent coke, is almost two thirds of the entire weight; of the volatile matter, which is about one third part, more than three fifths are combustible, and in a form to act very advantageously in producing a bright and hot blaze, while only one thirteenth part of incombustible, earthy and metallic matter remains in the form of ashes. This proportion of incombustible matter is a positive advantage, for being a bad conductor, it makes the fire hotter by retaining and accumulating the heat. Count

Rumford caused balls to be made of clay and fine coal moistened and kneaded together, the object being not only to economize the waste coal, but also to accumulate and radiate the heat.

As in the Mid Lothian coal there is very little iron, it is not likely that the ashes will readily form slag or clinker to obstruct the bars of a grate, or to accumulate like a fungus, upon the walls of a furnace.

Should there be occasion to convert the Mid Lothian coal into coke, it would afford that very important fuel of an excellent quality. If the process were conducted at a low heat, it is probable that a very brightly burning gas would be obtained, fitted for illumination, especially if it were mixed with a requisite proportion of the gas from rosin, as is done in the gas works in Boston, where Pictou coal of Nova Scotia is employed for this purpose. The Mid Lothian coal contains so little sulphur, that for every practical purpose it may be regarded as free from that combustible which is so injurious to the working of bar iron and steel by the forge and hammer, especially in the very important operation of welding. From repeated trials made with the Mid Lothian coal by our smiths in this city, it appears perfectly well adapted to their uses, especially where a hollow fire is desired, and when a powerful heat is necessary for large work with a strong blast. One of our best smiths, having made a comparative trial of the two, remarks, that it does not ignite as soon as the Newcastle coal, but gives a surer good welding heat, and lasts nearly one quarter longer.

This coal is an excellent fuel for a parlor grate. No bitumen exudes during its combustion; on breaking a heated mass by the poker, there is no liquid tar covering the separated fragments, but a bright flame instantly kindles on the newly exposed surfaces, which radiates heat powerfully and illuminates the room with a cheerful radiance.

There being no liquid bitumen, the combustion of this coal is attended with less smoke than is usual with bituminous coals; with a well drawing vent, there is scarcely a perceptible odor and no deposit of coal dust in the room and upon the furniture.

From a considerable experience in using it by us in a family parlor, it proves to be a very desirable fuel. We presume that it would prove an excellent fuel for locomotives and for steam engines, as it is abundant in flame so important to the production of

steam, while its coke maintains a solid ignited mass, ready at all times for the renovation of the activity of the blaze on the addition of more coal or of wood.

In a grate it burns very well when mixed with the anthracite, and the fire is active, cheering and enduring.

The Mid Lothian coal, being remarkably free from pyrites, there appears to be no serious danger of its producing spontaneous combustion—an accident which, in the case of mineral coal, is generally attributed to the fermentation of pyrites; the sulphur and the iron both attracting oxygen from water, as well as from the air in the interstices of the coal, until it becomes ignited. It should not be forgotten, however, that many combustibles besides coal, are liable to spontaneous combustion, and therefore care is always to be observed in disposing of them in store-houses, on ship-board, &c., especially when accumulated in large quantities.

From the absence of sulphur, we should think this coal well adapted to the manufacture of bar-iron, and that in employing it for locomotive engines and the boilers of steam-ships, or of fixed establishments on shore, there can be no cause to fear that it will injure the metal, whether of iron or copper.

As to its use in sitting and sleeping rooms, there can be no injurious influence to health, provided there is a good draught up the chimney; otherwise every species of fuel is dangerous, as the gases produced by combustion are all deadly; but, with a good drawing vent, there is no more danger from the Mid Lothian coal than from any other, and no danger indeed from any.

It is worthy of remark that whenever a coal fire becomes languid on account of the discharge and consumption of the gas, a billet or two of wood instantly renews its activity and prepares it for the reception of more coal, which is then promptly kindled.

Presuming, of course, that the coal furnished to us by the president of the Mid Lothian company, presents a fair average of the produce of the mines, we hesitate not to recommend it as an excellent fuel, which has no occasion to shun a comparison with the best mineral coal of this country or of Europe.

Yale College Laboratory, Feb. 7, 1842.

Art. XX.—*Bibliographical Notices.*

1. Caroli Linnæi *Systema, Genera, Species Plantarum uno volumine. Editio critica, adstricta, conferta; sive Codex Botanicus Linnæanus, textum Linnæanum integrum ex omnibus Systematis, Generum, Specierum Plantarum editionibus, Mantissis, Addimentis, selectumque ex ceteris ejus botanicis libris digestum, collatum, contractum, cum plena editionum discrepantia exhibens: In usum Botanicorum practicum edidit brevique adnotione explicavit* Hermannus Eberhardus Richter, M. Dr. Prof. Dresd., etc. Leipsic, (Wigand,) 1840.—This book is, as its title denotes, a complete digest of the writings of the immortal Linnæus upon systematic botany, an undertaking of great labor, and, we believe, very faithfully executed. It forms a volume of 1100 pages of the small folio or imperial octavo size, (the same as that of the new edition of Steudel's Nomenclator,) closely printed in double columns; prefaced by some critical and explanatory editorial observations, and by a complete list of the botanical writings of Linnæus, with notices of the different editions, a catalogue of the authors cited by Linnæus, &c. The prefaces, dedications, and introductory observations of all the systematic works are next given; and the body of the work is devoted to the genera and species, in which, by a well arranged system of abbreviations, nearly the whole Linnæan text, and the changes or variations of the different editions, are brought within a moderate compass. Such a *thesaurus* is of great value to botanists, and especially to those who do not possess the original editions of all the works it comprises, many of which are exceedingly rare. To the volume is appended a complete index to the Linnæan genera and species, with all the original synonymy, entitled: *In Codicem Botanicum Linnæanum Index Alphabeticus, Generum, Specierum ac Synonymorum omnium completissimus, composuit atque edidit* Dr. G. L. Petermann, which is paged separately, and occupies 200 pages, printed in triple columns, extending the work to above 1300 pages. It is published at 16 Saxon thalers.

2. *Genera, Species, et Synonyma Candolleana, alphabetico ordine disposita, seu Index generalis et specialis ad A. P. De Candolle Prodromum Syst. Nat. Regni Vegetabilis:* auctore H. W. Buek, M. D. (Berlin.)—An index of the genera and species contained in the Prodromus of the lamented De Candolle, and of their synonyms, has been greatly needed, those of the several volumes of that most important work extending only to the genera. This want Dr. Buek has in part supplied by publishing an index to the fifth, sixth, and first part of the seventh volumes of the Prodromus, that is, of the immense family of

the Compositæ. It is comprised in 223 pages octavo, (Berlin, 1840,) and is entitled the second part of the work: the first, an index of Vols. I–IV, of the Prodromus, although announced as in press a year or two since, has not yet reached us.

As to the Prodromus, although the gifted author was not spared to finish his herculean task, it will doubtless be continued, and, we trust, duly completed, by his justly distinguished son and successor, Prof. Alphonse De Candolle, with the aid of those botanists to whom a considerable portion of the remaining orders have from time to time been assigned. It may perhaps be important to the botanists of this country to know, that the elaboration of the *Scrophularineæ*, *Labiatæ*, *Hydrophyllaceæ*, and, we believe, the *Polemoniaceæ*, has been long since undertaken by Mr. Bentham; the *Convolvulaceæ*, by Prof. Choissy, of Geneva; the *Primulaceæ* and *Lentibulaceæ*, by Mr. Duby; and the *Plumbaginaceæ*, by Mr. Boissier, of Geneva; the *Solanaceæ*, by Prof. Dunal, of Montpelier; and the *Asclepiadeæ*, by Mr. Decaisne, of the Royal Museum, Paris; to all of whom good specimens of the rarer or less known and local species of these respective orders from different parts of this country would doubless be welcome and very useful.

3. *Kunth, Enumeratio Plantarum*, Vol. III. Stuttgardt, 1841. pp. 644, 8vo.—We learn that the third volume of this work has recently appeared; and that it comprises the orders *Araceæ*, (including *Lemna* and *Pistia*,) *Typhaceæ*, *Pandanaceæ*, *Naidaceæ*, *Juncagineæ*, *Alismaceæ*, *Palmaceæ*, *Juncaceæ*, *Phylidraceæ*, *Restiaceæ*, *Desvauxiaceæ*, and *Eriocauloneæ*.

4. *Loudon's Arboretum et Fruticetum Britannicum abridged: or the hardy trees and shrubs of Britain, native and foreign, scientifically and popularly described; with their propagation, culture, and uses in the arts, and with figures of nearly all the species: Abridged from the large edition in eight volumes, and adapted for the use of Nurserymen, Gardeners, and Foresters.*—This useful and well digested abridgment of a very important, but somewhat unwieldly and expensive work, is to be comprised in ten monthly parts, published at five shillings each, and will contain many species or varieties introduced into Great Britain since the year 1838, when the large work was completed. Only the first part (published in December last) has as yet reached us: this extends to p. 128, and includes the orders from *Ranunculaceæ* to *Æsculaceæ*, following the arrangement of De Candolle's Prodromus. The original work is highly and justly valued in this country, as well as in England; and the extremely moderate price of the new and abridged edition will doubtless secure for it a very extensive circulation.

5. *Steudel's Nomenclator Botanicus*, 2d edition.—We noticed this work in a recent number of this Journal, (Vol. XLI, p. 373,) while in the course of publication: the remaining fasciculi (XI–XIII) have since been received, which complete the work. It enumerates six thousand two hundred and eighty two genera, and seventy two thousand four hundred and seventy eight species of Phanerogamic plants.

6. *Torrey and Gray's Flora of North America:* Vol. 2, part 2. March, 1842. This number, as well as a large portion of the preceding, is occupied with the Compositæ; and this vast family is not yet finished; but will apparently require at least half of the ensuing number for its completion.

7. *Mr. Nuttall's Edition of Michaux's Sylva Americana.*—We are informed by Mr. Dobson, the publisher of this work, that it is at length definitively finished, in six volumes, imperial octavo, with 278 plates. Mr. Nuttall's additions can be had separate in three volumes, containing 122 plates, to complete *all* former editions of Michaux's Sylva.

This labor of Mr. Nuttall is looked for with great interest by all, and when it appears on our table will be the subject of further notice.

We also learn from the same source, that the first volume of the *revised edition* of Holbrook's North American Herpetology is also in the press.

8. *Botanical Teacher, Second edition;* by LAURA JOHNSON.* (Second notice.) In 1834, the first edition was published under the supervision of Professor Eaton. It was dedicated to the Hon. Stephen Van Rensselaer, and received particular marks of his favor and patronage. In the present edition improvements have been made, and it is particularly prepared for the pupils of teachers, who use the eighth edition of Eaton's North American Botany. The last named work having grown to a large octavo of more than six hundred close pages, teachers were in want of a cheaper book, to put into the hands of pupils. Such a book was found to be very difficult to construct. It was necessary that it should be plain—though it must be technical and truly scientific—and contain all the genera and species of North American plants, excepting the lower orders of Cryptogamia, and so much of these orders as might be needed in students' exercises.

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* Dr. Gray's notice of this book on page 184 of the present volume, having given dissatisfaction to the authoress and to Prof. Eaton, we have been requested by Prof. E. to publish the above, drawn up by himself. Miss Johnson's work is before the public, and they will judge of it for themselves.—EDS.

The Rev. Mr. Phelps had prepared a book of this kind, to accompany the British Flora of Dr. J. E. Smith, President of the Linnæan Society of London, which was well received. His method was adopted by Miss Johnson, with some amendments. The Botanical Teacher gives Lindley's concise generic descriptions of the genera, without abbreviations; but the specific descriptions are given by abbreviations. By using but one set of words, a general system of North American plants is compassed in a small volume of 268 pages.

This treatise is universally approved by all correct teachers of botany, who have seen it. On a hasty view, the abbreviation plan may appear forbidding. But by a card properly adjusted, the reader sees every abbreviation at one glance of the eye, without opening the book.

Being prepared by an experienced teacher for the use of her own pupils, and for the general extension of the science among young scholars, (for whom she considers botany as better adapted in early youth than any other study,) nothing is charged on the work for authorship. Therefore a class of a dozen pupils can be furnished for about half as many dollars.

As it is fitted for the vest pocket, and contains all North American plants, (excepting some recent discoveries in California and other distant regions,) it is most perfectly adapted to the wants of experienced botanists, who collect plants in fields and forests.

Errors, misprints, and omissions are to be found in it as in all books. But considering the great care and labor required in reducing a general system of the botany of a continent to a book of a hand's breadth and thickness, the errors are very few.

9. *Monographie d'Echinodermes vivans et fossiles, par L. Agassiz.* 2d livraison, contenant les Scutelles.

M. Agassiz's Monograph of the Echinodermata, living and fossil. 2d livraison, comprising the family Scutella, (Linn.) 4to. pp. 131, and 27 plates. Neuchatel, July, 1841.

In Vol. xxxvii, p. 369, of this Journal, we announced the appearance of the first livraison of this work, and gave an abstract of its contents. That part, it will be remembered, was devoted to the family of the Saleniarii, and a conspectus of the genera and species of that family will be found in the notice alluded to. The present livraison embraces that part of the family of the Clypeastroides containing the Scutellarii. It is prefaced by an interesting chapter on the history, different divisions, general form, structure, relations to other Clypeastroides, and geological and geographical distribution of this family.

In twenty seven elaborate plates, in part colored, we are presented with about two hundred and thirty distinct figures, including enlarged

parts, and the descriptive text is full, and accompanied with a copious synonymy and references to other authors. We regret that our present limits do not permit giving a full conspectus of the genera and species of this group; but we must content ourselves with giving only the genera and the number of the species under each.

I. Rotula, (Klein,) 2 species. II. Runa, (Agass.,) 2 species. III. Millita, (Klein,) 5 species. IV. Encope, (Agass.,) 11 species. V. Lobophora, (Agass.,) 4 species. VI. Amphiope, (Agass.,) 2 species. VII. Scutella, (Lam.,) 12 species. VIII. Echinarachinus, 4 species. IX. Arachnoides, (Klein,) 1 species. X. Scutelleria, (Agass.,) 5 species. XI. Laganum, (Klein,) 14 species. XII. Echinocyamus, (Agass.,) 11 species. XIII. Moulinia, (Agass.,) 1 species.

Like all the works of this distinguished author, the present livraison is marked by its great fidelity and the beauty of its mechanical execution; and our constant wonder is, how Prof. Agassiz can carry on at once so many great works as we know he has in hand, and yet devote to each a measure of labor which few other naturalists can command for a single object.

We beg again to call the attention of American naturalists to the request of M. Agassiz, that all who are so disposed, will send him specimens of the Echinodermata of America, for which due acknowledgment may be expected.

10. *Boston Journal of Natural History. Published by direction of the Boston Society of Natural History.* Boston: Little & Brown, 1842. Vol. IV, Part I. pp. 136, with 7 plates.—This part contains the following papers:

Art. I. Dissection of two adult dromedaries, a male and a female, by J. B. S. Jackson, M. D. II. Descriptions of the Fishes of the Ohio river and its tributaries, by J. P. Kirtland, M. D. III. Observations on the genus Scalops, (Shrew moles,) with descriptions of the species found in North America, by J. Bachman, D. D., Charleston, S. C. IV. On the occurrence of the Phosphate of Uranium in the Tourmaline locality at Chesterfield, by J. E. Teschemacher. V. Descriptions of twenty four species of the Shells of New England, by J. W. Mighels, M. D., of Portland, Me., and Prof. C. B. Adams, of Middlebury College, Vt. VI. Descriptions and figures of the Araneides of the United States, by Nicholas Marcellus Hentz. VII. Descriptions of two new species of Fishes, by D. Humphreys Storer, M. D. VIII. On a new species of Rafflesia from Manilla, by J. E. Teschemacher. IX. Remarks upon Coral Formations in the Pacific, with suggestions as to the causes of their absence in the same parallels of latitude on the coast of South America, by Joseph P. Couthouy. X. Niagara Falls—their physical

changes, and the geology and topography of the surrounding country, by James Hall. XI. Note to the editors respecting Fossil Bones from Oregon, by Henry C. Perkins.

A glance at this list will show that the present number of this Journal is more than usually rich in subjects of important and general interest to all naturalists. It speaks alike of the thrift of the Society of which it is the organ, and of the zeal and ability of its members.

11. *Report on the Insects of Massachusetts, injurious to Vegetation. By* Thaddeus William Harris, M. D. *Published agreeably to an order of the Legislature, by the Commissioners on the Zoological and Botanical Survey of the State.* Cambridge, 1841, 8vo.—We have not yet had an opportunity to examine this important work, but from our knowledge of the eminent qualifications of the author, we are confident that the book is one of great value, alike to the intelligent agriculturist and to the scientific inquirer. The commonwealth of Massachusetts has earned for herself much honor, throughout the learned world, by her liberal patronage of science; besides which she will doubtless receive in the increased resources of her own people, an abundant pecuniary recompense. We hope to be able to speak more particularly of Dr. Harris's Report, at some future day.

12. *Publication of Rogers's Letters on the Manufacture of Iron;* by J. H. Alexander, Esq., with an Appendix.—Will shortly be published, under the editorship of Mr. J. H. Alexander, of Baltimore, "Letters on the Manufacture of Iron," by Samuel Rogers, of Monmouthshire, South Wales.

Of this book, a notice appeared in 1829, in the preface to the Manuel Complet du Maître de Forges, by M. Landriu, of Paris, in the following words:—

"C'est dans cet état de la question,"—namely, after M. Landriu, having completed the list of metallurgic writers anterior to the reformation of the phlogistic theory, has farther illustrated the subject by reference to the systematic and learned labors of Hafrenfratz, the immense scientific and practical knowledge of M. Karsten, and the supplementary critical memoirs of M. Müller,—"que Samuel Roger de Risca, métallurgiste aussi éclairé que modeste, rédigeait en Angleterre son Traité du Fer (an Elementary Treatise on Iron-making, 1819) dans les usines mêmes où il ne craignait par de manier le doli du puddleur. Il y exposait avec clarté et simplicité les principes scientifiques de la Sidérurgie; montrait qu'on pouvait extraire le fer à l'état de pureté de toutes les matières dans lesquelles il était combiné, avec tous les combustibles qui avaient le carbone pour principal élément; et fai-

sait voir à quelles substances le fer devait sa propriété de devenir cassant," etc. etc.

"Cet ouvrage devait faire la matière de trente lettres in folio, dont Roger fit imprimer les deux premières afin de se procurer des souscripteurs. A l'annonce de cette publication et à la lecture de l'introduction dans laquelle le plan en était savamment developpé, la terreur s'empara des maîtres de forges Anglais : ils craignirent que le savant chimiste ne portât la lumière dans une carrière où ils avaient soin d'entretenir l'obscurité ; ils résolurent d'étouffer ce beau génie et accoururent en foule dans le Monmouthshire pour racheter au prix de l'or un monopole qui allait leur échapper. Roger eut la faiblesse de céder aux offres de ces avides Bretons et ses élucubrations restèrent enfouies dans les cabinets de trente personnes intéressées à les cacher de tous les yeux."—*Landriu*, *tom.* I, pp. 11 and 12.

With less of the somewhat theatrical pomp under which M. Landriu saw fit to introduce his notice, another, grounded upon the careful perusal of the said thirty letters and personal enquiries among those under and with whom Rogers had worked, was made by Mr. Alexander, in his Report on the Manufacture of Iron, noticed in Vol. XLI, No. 2, of this Journal.

Under these concurring testimonies there is reasonable ground for believing that the book will be found to contain matter of importance for all who are interested in the subject.

Mr. Alexander stands in no other light with regard to the publication than that of friendly editor, as we are informed ; adding nothing of his own except a review of the experiments on the expansibility and point of fusion of this metal, and the results of his own experiments on the fusibility of different earthy and metallic silicates which are found in or may advantageously enter into the composition of the furnace cinder or slag.

The design of Mr. Alexander in taking the trouble of this publication was, as well to aid the family of Rogers—some of whom are understood to be struggling in obscure poverty somewhere in Wales—as in furtherance of a *corpus* of treatises on the subject, which he proposed to publish in the interest of this most important branch of American manufactures, under the general title of "Contributions to the History of the Manufacture of Iron ;" to which his Report, &c. before mentioned, was meant to serve for introduction.

In the introduction to that report he mentions Rogers and his work in the following terms :

"In 1819, Samuel Rogers, a working hand about one of the establishments in Monmouthshire, but in many regards an extraordinary person, had yet, by some means, acquired a very judicious comprehen-

sion of the aim and application of the science of chemistry; and several of the remarkable discoveries of the last fifteen years in this manufacture, are to be found, either in germ or more distinctly brought out, in certain letters, which, during the year mentioned, he wrote and proposed to publish. There was reason to suppose that the effect of his views, if adopted, would have tended to equalize the proportionate products of establishments of different sizes, and possessing different natural advantages; but the interest of the large and favorably situated manufactories was not to encourage this equalization, or, as they thought it, rivalry; and by temptations of whatever kind, Rogers was induced to give no more than his first three letters to the public. But, a few copies of this work as he prepared it, still exist in manuscript, and one of them is now in my possession. Upon a careful perusal, I cannot but think that the iron-masters overrated the influence which the entire publication would have had; and Rogers was, perhaps, acute enough to come to the same conclusion. However, it would have been unjust in any treatment of the same subject, to have withheld the honorable mention of himself and his work, which I have thought proper here to make."

In Vol. XLI, p. 376, we inserted a brief notice of the labors of Mr. Alexander for the diffusion of correct information, both historical and practical, upon the manufacture and uses of iron, and we then gave an outline of his Report to the Governor of Maryland, upon this most important subject.

The publication named at the head of these remarks, forms a second step in the series of elucidations which we are authorized to expect, and for which Mr. Alexander, (an *unpaid* laborer in these important researches,) will impose upon his country a large debt of gratitude. No person in these states has undertaken such a labor, and all who are able, in consequence either of their scientific or practical knowledge, to contribute to the great result, will we trust be forward to sustain an enterprise of such magnitude, and connected with so widely diversified and momentous interests. We rejoice that the work has fallen into the hands of a gentleman so well qualified and so zealously disposed for its effectual performance. We understand, that in the current season, Mr. Alexander will lay before the Legislature, a statistical account of the manufacture of iron, as it now exists in Maryland; giving, as nearly as can be ascertained, particulars relating separately to high furnaces, foundery cupolas, and establishments for bar and plate iron; showing also how many of each are in activity, the fuel and raw material required by each, the number of men employed, the amount expressed in the scale of some unit of calculation of steam and water power, the quantity and value of the products of each, &c. &c. It is extremely desirable to have similar results obtained in all the Northern and East-

ern States, and especially in Massachusetts and Connecticut, in which states not only much of the coarser forms of iron, but of cutlery also, is manufactured. We are given to understand that Mr. Alexander's third number in his series on iron is in progress, and that it will present the exposition of his microscopic researches into the crystallography of crude iron.

MISCELLANIES.

FOREIGN AND DOMESTIC.

1. *Protest of Mr. Charles V. Walker.*

Editorial Remarks.—It is with much reluctance that we give publicity to the remarks of Mr. Walker, as it is extremely desirable, in matters of science, to avoid personal controversy, and we are not sure, that in the present case, the blame is not in part our own. The truth is, we were in doubt whether the letters of Mr. Sturgeon, referred to by Mr. Walker, were intended for publication or not. The subject-matter seemed to justify if not to require it, and we were ignorant of any personal claims that interfered. Still, the letters were retained in hand, in the hope of hearing farther from Mr. Sturgeon, and they were at last published so late, that it seemed as if an apology was due for their delay.

If we have exposed Mr. Sturgeon to criticism, by publishing what was intended to be private, we sincerely regret it; and on the other hand, Mr. Walker may feel that he has cause to complain that his remonstrance has not appeared sooner. Being friends of peace, we have been hoping to hear from Mr. Sturgeon or Mr. Walker, that the claims of all parties were satisfactorily arranged; but as we have no such information, we cannot act impartially (as it appears to us after much consideration) without giving Mr. Walker's own statement of the case—and we are not sure, after all, that we have not taken the course that will fail to give satisfaction to any of those concerned or to the public.

TO THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE.

Kennington Grammar School, Feb. 1, 1841.

Gentlemen—In this Journal, Vol. xxxix, pp. 28–36, is an article relative to some experiments made with an extended series of the constant battery, containing extracts from two letters addressed to you by Mr. William Sturgeon, in the latter of which that gentleman has labored hard to connect himself, to the exclusion of those who experimented with him, with a certain important experiment—the heating of the positive electrode beyond the circuit. Had he confined his observations

to that periodical of which he is the editor and proprietor, (the Annals of Electricity,) they might have rested undisturbed on my part; but when he publishes this new version of the affair in another quarter of the globe, selecting as a vehicle a journal of such established reputation as yours, whose pages are read wherever science is cultivated, and urges as a reason for publishing this new version, the want of clearness with which my account (as read before the London Electrical Society) was drawn out, I feel that I should be wanting in justice to myself and those who were with me, if I suffered it to pass unnoticed. With respect, first, to his charge against me of want of clearness; I shall not attempt to confute this, but refer your readers to his description on page 31, and mine (which you have copied verbatim) in pages 33, 34; and if a comparison is drawn between these, and it should appear that mine is deficient, though I confess I am at a loss to discover in what, be it so: *palmam ferat qui meruit.* There is one thing most assuredly conspicuous in *his*, which, he may think—though he should have thought so before, when he corrected the manuscript and the proof sheets, for they were all submitted to his inspection—is not recognized in mine; I allude to the frequent recurrence of the pronoun *I*. The account I drew up was descriptive of a series of experiments, carried on by Messrs. Gassiot, Mason, Sturgeon, and myself, at the house of Mr. Gassiot, and at his SOLE expense. The sole object was to advance the interests of science, through the medium of the London Electrical Society, and not to found individual claims to individual experiments, when each by agreement was contributing his own share to the common stock; you may judge, therefore, of the surprise with which I saw the experiment in question, not only claimed by Mr. Sturgeon as his, but also as being undertaken from certain views which he had long entertained. If he had entertained these views, he had a marvelous manner of concealing the experiments he had based on them; we, in our innocence of what good things were in store, were plodding on through that extended series of experiments on decomposition, with such a battery as had *never* been excited before, and yet our chief man (for he was the only scientific man by profession among us) is unable to avail himself of the first opportunity that *ever* occurred to him of bringing his views to the test. Only a *few* of his experiments were attempted, he says. If you, gentlemen, were personally acquainted with Mr. Gassiot, and had seen, I will not say the *liberality* only, but the *ardor* with which he encourages every attempt at experimental demonstration, you would wonder what change could have come over him, that he should have left Mr. Sturgeon's experiments *last* on the list. But granting that this experiment was peculiarly his, surely it was strange to leave it unadopted for so many months; he did not claim

it at the very outset, neither did he when I sent the manuscripts for his revision,—it passed as a portion of the *joint* stock when the whole was laid before the Society, and he allowed it to pass through the press and be published without asserting any claim. Nor am I aware that he attempted to appropriate it, until M. De la Rive drew attention to its importance, by endeavoring to repeat it. The want of success which attended M. De la Rive's endeavors, Mr. Sturgeon attributes to my faulty description, and this affords him a plausible pretext to lay his own version before the American public, lest they also should fail from a like cause. I would gladly know what there is in my description which prevented M. De la Rive from producing the same results. Surely that philosopher is not to be charged with deficiency of intellect and want of skill in manipulation; it requires very little of the former to comprehend the description I have given, and no large share of the latter to follow it. If you will refer to the Proceedings of the London Electrical Society, (a copy of which is forwarded to you by the order of the committee,) you will find on page 167, an abstract of a translation of M. De la Rive's experiments, and will see from that, that he perfectly comprehends me, but fails on account of the battery he used.

From this you will see that the motives assigned by Mr. Sturgeon are merely *imaginary*, but if *real*, they little became *him*—they should never have fallen from *his* pen, because, after the experiments were finished, the notes were offered him to prepare, but he declined them; and when I, at the request of the others, undertook the task, I sent the prepared manuscripts to Mr. Sturgeon, as well as to the rest, for his corrections or observations, if he had any to make; and they were returned from him with some emendations, but with no remark in connection with this experiment. Surely when he tells you that on account of the lateness of the hour many of his experiments were not entered into, he might have said that the battery was charged *three* different times, at each of which he was present, and on each of which there must have been opportunity. I am surprised that in a joint undertaking like this, he should talk of *his* experiments, as distinct from those of the rest, but still more so, when these were kept secret from us.

With regard to the experiment in question, it appears to have resulted, like many others in all the sciences, from merely fortuitous circumstances. He and Mr. Mason were amusing themselves with the wires, and observing the length of the arc of flame, and the phenomenon of the heated electrode presented itself; but neither knew which electrode it was until they had examined. And this, I think, you may gather from Mr. Sturgeon's own words in his first letter, dated October 9, 1838, where he says—"the wires were made to change poles, *still* the same thing

occurred." Why were the wires changed, unless with the impression that a particular something, connected with the nature of the wire, might be concerned in producing the effect? I cannot pass over the letter from which I take that extract, without remarking on the great want of courtesy on Mr. Sturgeon's part in sending you an account of experiments made for the Electrical Society, the date of his communication being a *week* antecedent to the day when they were read before the Society.

In conclusion, I would advert to a *slight* error into which Mr. Sturgeon must have fallen in his over-anxiety to be *correct:* he tells you the zinc was *amalgamated;* lest your readers should, in preparing a battery of this kind, be led to incur the trouble and expense of this, I would remind them that the zinc was in the condition in which we received it from the workmen.

With every apology for trespassing so much on your time and valued pages, believe me, gentlemen, your obedient servant,

CHARLES V. WALKER.

2. *Mineralogical Notices, by Dr. Lewis Feuchtwanger.*—The indefatigable mineralogist, Breithaupt, has, according to Berzelius's annual report for 1839, discovered eight new minerals: viz.

1. Trombolite, (θρομβος, numb, stiff,) a phosphate of copper resembling an opal from Retzbanja, Hungary, of a sp. gr. = 3.38 to 3.4; is of green color, opaque, and conchoidal, vitreous fracture; according to Plattner's analysis, it appears to have the formula $\dot{C}u^2\dddot{P}+16\dot{H}$.

2. Allomorphite, a sulphate of barytes, containing 2 per cent. of sulphate of lime, of papillary form, and found in an ochre mine near Unterwirbach, Duchy of Schwarzburg.

3. Anauxite, (αναυξης, not growing larger,) from the highlands of Bilin, of volcanic formation, resembles in appearance the Pyrophyllite, but on heating does not swell but peels off; is translucent on the edges, dark greenish white, fine granular, foliated fracture, sp. gr. 2.264 to 2.267. Contains silica 55.7, and water 11.5; the balance is alumina, calcia, and protoxide of iron.

4. Polyhydrite, a silicate of oxide of iron from Breitenbrun, Saxony, is of a hepatic color, vitreous lustre and opaque, sp. gr. 2.1 to 2.142; contains 29.2 per cent. of water.

5. Serbian or Miloschin, forms a protruding layer in a mountain in Servia. Serbian is blue or bluish green, acquires a lustre on rubbing, opaque, conchoidal fracture, and sp. gr. 2.131; it crumbles by water with a noise; it contains principally alumina, less silica, oxide of chrome, a trace of magnesia, and 22.8 water.

6. Violan, a silicate of alumina, magnesia, lime, much protoxide of iron and soda, and occurring at Piedmont with manganesian epidote; has waxy lustre, deep violet blue color, nearly conchoidal fracture, amorphous, opaque, uneven, brittle, sp. gr. 3.233, does not change on heating, but may be brought by a higher temperature to a clear bead.

7. Tombacite, an arsenical nickel ore, with a little sulphur, and small trace of iron or cobalt, occurring near Lobenstein in Voigtland; in color it resembles the magnetical iron, sometimes with a greenish brown hue; its streak is black, appears to belong to the hexahedral system, is brittle, non-magnetic, sp. gr. 6.637.

8. Hepatic blende, a mineral mostly wax-yellow, from Saxony, in the mine Hochmuth near Geier, Himmelreich-Erbstollen, between Marienberg and Wolkenstein, and also from Cornwall. The color varies from pea yellow to pink brown, transparent; the streak is either colorless or yellowish gray, forms botryolitic and reniform conglomerates, fracture conchoidal, and a sp. gr. 3.7 to 3.78, and, according to Plattner's experiments, is said to be a sulphocarbonate of zinc, it containing zinc, sulphur, and carbon. It decrepitates on heating, yielding water and a little sulphur, smells like sulphuretted hydrogen, and then like coal tar, and then becomes gray; it is decomposed by hydrochloric acid, disengaging sulphuretted hydrogen; the gray substance remaining from before is soluble in nitric acid, leaving sulphur and carbon, the first of which may be sublimed and separated.

It may be inferred from the experiments of Plattner, that this mineral consists of sulphuret of zinc formed by water, and intensely mixed with bitumen or other carbonaceous compound; for it is not to be presumed to contain any carburet of sulphur, which would in those instances distil over unchanged, unlike the above.

Hess has described a new mineral, which he calls *Volborthite*, consisting of vanadiate of copper, of yet undetermined degree of combination. It forms crystalline needles of olive green color, papillary; is translucent in splinters, has a yellowish green streak, and a sp. gr. 3.55; on heating grows black, yields a little water; it melts before the blowpipe, and by increased heat yields a slag like graphite, extending upon the charcoal with some metallic copper; by soda the copper is reduced instantly, and vanadious soda is formed.

Gigantolite,* by Nordenskiöld, from Tammela, Finland. One of the crystals of that mineral measured two and a half inches in diameter. This mineral resembles the Fahlunite, and all the harder varieties of

* What name could we give to our gigantic crystals of beryl, topaz, apatite, tourmalines, zircon, rhomb-spar, lead, fluor-spar; the four latter from the State of New York, some of which measure twelve to fifteen inches in diameter?

talc; it has been described and analyzed by the Count Trolle Wachtmeister; its color is steel gray to brownish, yields on heating, water with some ammonia; it contains silica 46.27, alumina 25.10, oxide of iron 15.60, magnesia 3.80, protoxide of manganese 0.89, potassa 2.70, soda 1.20, water 6.00, and a trace of fluor, and has a formula of $\dot{R}\ddot{S}i + \ddot{A}l\ddot{S}i + \dot{H}^4$.

3. *Infusorial Animals.*—Baron Von Humboldt presented to the Academy, from M. Ehrenberg, of Berlin, specimens of the argillaceous and peaty formation found beneath the city of Berlin, at twenty feet under the surface. It was full of small infusorial animals, all alive, with living ovaries, and capable of reproduction. He had discovered similar formations in other parts of Prussia; and he mentioned as a curious fact, that of 1,728,000 cubic feet of matter taken out of the port of Swinemunde, on the Baltic, in 1840, one half of it was composed of microscopic beings. The sandy plains of the Lamburg contained strata of fossil infusoria twenty eight feet thick.—*Literary Gazette, Nov.* 13.

4. *Coal Mines in Cuba.*

To the Editors of the American Journal of Science and Arts.

Gentlemen—In the belief that no account has appeared in any American publication, of the extensive coal mine which has been discovered in Cuba, the progress made in the examination of which I have for a year or two past watched with much interest, I send herewith a notice published by *M. Castáles* in the "*Diario de la Habana*," of the 7th of August. The mine is situated in the Partido de San Miguel, about six miles from Havanna, and is particularly interesting on account of its locality and the quality of the mineral.

The coal is of two kinds, one of which, denominated "*chapapote*," is the most abundant. One hundred parts of this yielded fifty parts of volatile matter, and afforded by analysis,

Carbon, - - - - - - - - - -	71.84
Oxygen, - - - - - - - - - -	6.22
Hydrogen, - - - - - - - - -	8.40
Ashes composed of silica, ox. iron, and sulph. lime,	13.51
	99.97

I remain, gentlemen, with great esteem and respect, your friend and servant. JOHN H. BLAKE.

Boston, October 7, 1841.

"The abundance and good quality of the coal are the two particulars embraced in this article to which we should like most assuredly to give a greater extension. Almost at the lower extremity of a hill whose inclination is not very steep, they have opened a rectangular well of four yards in superficies, and eighteen in depth, and at one yard excavation they met the coal, which continues to the above mentioned depth, the quality of the ground being, as well at this point as in the others, a calcareous and ferruginous layer. At the distance of forty five yards up the declivity, they have opened another well, three yards wide, two broad and forty deep: in this place, the coal was found at the depth of seven yards, and continues to the bottom, at which point and in the center of it, they made a bore of fifteen yards, always meeting with coal. At the four sides of the bottom, they have opened a straight gallery, thirty yards in length, in which the vein continues horizontally without any interruption. In this well, terminates another gallery, which opening from the bottom of the other, communicates with this, the drain being obtained by means of oxen.

"On the road to Tapaste, and on the summit of the hill, at a distance of four hundred yards from the preceding well, they have opened another, the vein of coal beginning at the depth of fourteen yards. It results then, that in the small space above mentioned, is found a vein of coal of forty eight yards perpendicular, and more than sixty in surface, in the part bored up to the month of April last, interrupted with layers of stone, and some spots of chalk, though of small extent and rare. The bed of coal is almost horizontal; the difference of the depth at which it is found, is one yard in the first well, seven in the second, and fourteen in the third, depending upon the variation of the surface of the declivity of the hill.

"The mine Prosperidad was examined by Mr. San Richard, an English engineer, who came to Cuba for this purpose: he wrote to the Society the following, which we take from a copy now under our eyes:—'Descending into the well, I became astonished at seeing such a vein of of coal; never have I seen or heard till now, that there is in other places a similar vein, and I believe that I should not be mistaken in saying that there are few persons who have seen another so extraordinary as this. The coal from the surface, to the depth of a few yards, appeared to me to be charged with bitumen, and a coal of very good quality for coke; that which I have seen made with it, is, in my opinion, of superior quality. From the above mentioned distance to that of forty or fifty yards that I descended, the quality of the coal changed much to its advantage; it is less bituminous, contains a greater quantity of oxygen, and is much more compact. I saw at the bottom of the well galleries opened to the four winds, to the length of twenty or thirty yards,

and it is all around full of coal. There is also at the east, a gallery a few yards from the bottom, to the extent of forty or fifty yards, all surrounded with coal, so that they see nothing else on all sides.' "

5. *Encouragement for the Fine Arts.*—George Combe, Esq. under date of March 16, 1841, writes to the senior editor of this Journal:

"I am glad to hear that Mr. Ives (sculptor and modeller in statuary) has obtained so much patronage among you. It appeared to me that there is no lack of genius for art in the United States; all that is needed is encouragement. Scotland was too poor to encourage artists by buying their works, until we formed an association, to which any one who chooses subscribes five dollars; we buy pictures with the funds, (last year they amounted to £3,000,) and draw lots for them. The annual exhibition has recently opened, and it is very creditable to the country. The improvement in art, within my recollection, is very great, and the public taste is improving in proportion. Such a scheme is what your country wants."

We hope that the valuable suggestion of Mr. Combe may be favorably regarded, both in the revival of institutions already existing for the improvement of the arts, and in the creation of new and effective associations.

Twelve months have passed since the above remarks were written, and they have lain among our unpublished miscellanies until we can have it in our power to confirm their justness and propriety.

6. *Geological Survey of Louisiana.*—We are happy to learn from Prof. Wm. M. Carpenter, of Jackson College, Louisiana, that he has for some time past been engaged in making, by direction of the legislature, a geological examination preliminary to a complete survey of that state. Prof. Carpenter is well known to the readers of this Journal by various interesting geological papers in our previous volumes, and we rejoice that the legislature of Louisiana have had the wisdom to select, from her own sons, one so able to answer their liberal views. From Prof. Carpenter's letter we extract the following.

Notice of an interesting Fossil.—The sketch represents the crown of a molar tooth, which was taken from a jaw bone found at the depth of forty five feet below the surface, in digging a well in a prairie twenty or thirty miles from the town of Opelousas, in the western part of this state. When taken up, the jaw bone is said to have been nearly entire, but was fragile, and soon crumbled, and as the discoverers saw nothing remarkable in the jaw except the circumstance of its being found at such a depth below

the surface, it was thrown away, and this crown was all that was saved. Description:—horizontal section of body quadrilateral, with the angles rounded and the sides slightly curved. The crown has two transverse ridges, the summit lines of which are slightly curved; between the extremities of the ridges on each side is a small tubercular elevation, and a slight elevation borders the anterior and posterior extremities of the crown.

Size of body, { length, 0.94 of an inch. breadth, 0.76 " "

Length of the summit of the ridges, 0.55 of an inch. Distance of the summits of ridges from each other, 0.42 of an inch. Height of ridges, 0.36 of an inch.

It is without doubt the fifth molar of the left lower jaw of a Tapir, which appears to me to be very near to the one now inhabiting South America, as the form and size of the tooth is nearly the same as in that animal.

Jackson, La., October 19, 1841.

7. *Preparation of Freshwater Shells for the Cabinet.*—We make the following extract from the letter of a distinguished correspondent, whose shells have been in much demand among collectors, and whose mode of preparing them is the result of observation and experience.

"It is well known that these shells are composed of animal matter and carbonate of lime, thinly laminated. Many of them are more or less covered with mucus, lime, clay and oxide of iron, sometimes indurated, so as to require a steel instrument to remove it. Hence the first operation is to remove this extraneous matter by hand-brushes, and then with dilute muriatic acid to remove the free lime and accidental colors; then, after a thorough rinsing, and as soon as the water has dried from the surface, saturate the shells with the finest spermaceti oil, which should be left on them for several months if convenient, but wiped from them as clean as possible with a woolen cloth before putting them in the cabinet. They will then feel like steatite, and exhibit a transparency and beauty which I could not obtain in any other way. Shells which have once been exposed to the air, without the animal, and have become thoroughly dry, can never be restored to their primitive beauty, because the water of the animal matter in them has evaporated. They become opake, and a slow decomposition, like that of salts, takes place, by the evaporation of the water of crystallization; but the oil taking the place of the water, as the latter evaporates, increases the transparency of the shell, as it does that of paper, and the superfluous oil may be so effectually removed at the proper time, that the shells will not soil the fingers or smell unpleasantly; but any considerable exposure to the air and light will soon injure their appearance."

8. *Bones of the Orycterotherium.*

Dear Sir—Dr. Perkins is under the erroneous notion that my remarks on the "Orycterotherium," in the Journal of proceedings of the American Philosophical Society, is a *description* of the new genus, whereas it is only intended as a scientific notice. My memoir before the Society, of twenty one pages letter paper, with numerous figures, is now in progress of publication.

The "*protuberance*" on the humerus referred to by Dr. P. is there noticed, together with numerous other details not now mentioned, and all of which leave no reason to believe Dr. P.'s bones to have belonged to a distinct species. He is certainly premature in giving the specific title of "*Oregonensis*" to his remains. R. HARLAN.

Philadelphia, Feb. 1, 1842.

9. *Note on Mr. H. C. Lea's paper in the last number of this Journal.*—Among some interesting additions to the known species of our native shells in the last number of your Journal, I find a shell described under the name of *Pasithea sordida*, which has been known to me for several years, and had been regarded as a variety of *Actæon trifidus, Totten.* A re-examination of numerous specimens confirms this opinion. The species presents the following varieties, the type being characterized by three well impressed and several indistinct revolving lines.

Odostomia trifida, Gould. Invert. of Mass., p. 274, fig. 179.

Actæon trifidus, Tott. Am. Journ. Science, XXVI, 368, pl. 1, fig. 4.

Var. a. With two well impressed lines.

" *b.* With one well impressed line.

" *c.* With all the lines obsolete.

" *d.* With one well impressed line, and the columellar fold indistinct.

" *e.* With the lines obsolete and the fold indistinct.

Pasithea sordida, H. C. Lea. Am. Journ. Science, XLII, 110, pl. 1, fig. 6.

Varieties *a* and *b* are most common in the vicinity of New Bedford. In most of the individuals, which would, at first, be referred to varieties *d* and *c*, the fold will usually be seen far within the aperture. But occasionally it is wanting, and a roughness of the columella indicates this to be the result of disease or accident. Without the intermediate varieties, *e* might be supposed quite distinct from the type, and many species have been proposed with much less reason. But having a large number of the shells referred to by Mr. Lea, among which are *all* the above varieties, I cannot regard it as entitled to specific rank.

Mr. Lea is in error in supposing that his shell and the *Cerithium Sayii*, Menke, (*C. reticulatum*, Totten,) among which it was found, are from Boston. Although in Col. Totten's description of the latter species, Boston harbor is mentioned as its habitat, it has not probably been found north of or within Cape Cod, its extreme limit being Provincetown, where it was found by Dr. Gould. The shells in question were obtained in Dartmouth, Mass., where they were clinging to the *Zostera marina* below low-water mark. Very respectfully,

C. B. Adams.

Middlebury, Vt., Feb. 15, 1842.

10. *Notice of some facts connected with a stroke of lightning, in a letter from Rev.* James H. Linsley,* *dated Stratford, Conn., Sept.* 9, 1841.

Prof. Silliman—*Dear Sir:* Early in June, 1821, four men, who had been engaged in fishing, were cleaning shad upon a plank ten or twelve feet in length, one end of which was resting upon the edge of a stump, and the other upon an empty flour barrel, the latter being towards the river. A large pile of the offals of shad was lying around the stump; a steel pointed pitchfork was standing by the plank, which, as well as the prongs of the pitchfork, was smeared with the fish-oil. A heavy shower had commenced, and the men took shelter in a shed about twenty five or thirty five feet off, when the lightning struck the stump, splitting it to pieces, until it came down to the fishes' entrails and heads that were piled around it. Below them it did not affect the stump or the ground, nor injure the plank, or the pitchfork by it on the barrel; but took the ground at the lower end of the barrel, and thence ploughed a furrow until it came to a rock about five feet in length or two or three feet horizontal thickness, weighing several tons, through which it passed, leaving one side broken in several pieces, and the other side unbroken, with a square face, as if sawed through. The rock is thinly laminated, but the lightning did not separate the laminæ; it cut across nearly at right angles, i. e. varying only twelve degrees, the laminæ being nearly perpendicular to the horizon. From the rock, the lightning passed to the water and disappeared. In a few moments, however, many dead fishes of various species rose upon the surface of the river; they appeared to come up "as they do when the ice over them in winter is struck by an axe." The effect upon the men in the shed was singular: one was seen from the dwelling house (about five rods distant) to stoop down as though picking up something with both

* The facts were communicated to Mr. Linsley by Mr. S. Crowfut, the owner of the place where the event occurred.

hands; he would then rise and extend both hands high in air, and then stoop down again as before; this action he repeated several times; at length he called to those in the house, saying, that "the lightning is so thick upon the ground that you can pick up corn-baskets full of it." His mind was evidently for a short time injured by the shock. Two of the other men, who had just sat down as the shock came, were found leaning back against the wall, stunned, as if asleep. The fourth received little or no injury.

The persons in the house, (Mr. C. believes about a dozen,)—most of whom had naked feet—said that at the moment of the shock their feet felt as though some person had tossed a chip of wood on them, while those with shoes on did not perceive this sensation.

In addition to this, an empty boat lay a short distance from the rock struck, and when the shower was over, the men who came there in the boat attempted to return in it, but on entering it immediately filled and sank. On examination it was found that every nail in the boat had started, and that the leaks were thus caused.

The points which I conceive of any importance in this transaction, are, 1st. The good evidence furnished, or the corroboration of a long known fact, that *oil is a powerful non-electric*, as the fluid passed over or under the whole length of the plank covered with the refuse of shad. 2d. The sensation given to all the *bare feet* of persons five rods distant, without affecting the hands and face, was uncommon. Is it not probable that the skin of the feet, being usually covered, was more delicate and therefore more sensible to the shock? The floor of the house where these persons were, is several feet higher than any point touched by the lightning. 3d. Did the electric fluid reach the fishes in the river? or were they killed by the mere shock in the air acting upon the water? 4th. Is it possible the nails in the boat could have been started out by the shock, and if so, in what manner? Was the concussion of air so great upon the plank of the boat, that the nails were thus drawn by the plank? or was this result produced by the electric fluid acting upon the nails? 5th. Is it possible to explain or show cause why the lightning should leave so smooth a surface through the rock which it severed, especially when acting not *with* nor directly *at* right angles to the natural cleavage or laminæ of the rock and not separating any laminæ?

Some person a short distance further up the river, who saw the column of electric fluid descend on this occasion, remarked, that "it appeared to be about the size of a common bar-post."

11. *Separation of silver or gold from lead.*

Prof. SILLIMAN—*Sir*: In looking over a former number of your Journal, (Vol. xxxv, No. 2, January, 1839,) I find on page 321 an

article on cupellation, where the writer proposes to separate silver or gold from lead by oxidizing the alloy in the external flame of the blowpipe on a slip of mica. This process is undoubtedly original with him, but a much better one has been practiced by me more than thirteen years, when I first learned it from Prof. H. Rose of Berlin.

Take a few grains of bone ash, make it into a paste with a little saliva, spread it about one line thick on a piece of charcoal, and make a shallow impression in it, to receive the globule of metal. Expose it to the heat of the blowpipe, so as to burn it white and hard, and then melt the globule of the alloy on it, and keep it in a constant red heat, till the lead is all oxidized.

The advantages of the bone ash over the mica are manifold. 1. It is easier to be obtained, and every where the operator can prepare a little if he should not be supplied with it. 2. The metal will remain in the concavity of the bone ash paste, and not be liable to run down and be lost, as on the mica. 3. It is never necessary to change the material; the bone ash absorbs the litharge which collects on the mica, and impedes the process, so that the remaining metallic globule has to be transferred to a fresh slip of mica. 4. The color of the paste, after the operation is finished, gives an indication as to the nature of some impurities of the metal; lead alone makes it appear yellow; a small proportion of copper changes this yellow color to greenish. Respectfully, your obedient servant, GEORGE ENGELMANN, M. D.

St. Louis, Jan. 22, 1842.

12. *Suggested observations relating to the total solar eclipse of July, 1842, visible in Europe.*—The sun is supposed to belong to the class of nebulous stars. The nebula that surrounds him is however, at ordinary times, very incompletely visible, being hidden by the effulgence which his reflected beams pour upon the eye from the atmosphere, and from the whole assemblage of terrestrial objects in the field of vision. It is only when this effulgence is withdrawn, and evening is far advanced, or the morning yet distant or scarcely beginning to glimmer, that this nebula may be observed in its remoter parts, lifting itself above the twilight, and forming the celestial phenomenon known commonly as the "*Zodiacal Light.*" At such times, however, the central body and the brighter regions of the nebula are concealed beneath the horizon.

Our only opportunity, therefore, for a complete observation of the zodiacal light, in its brightness near the sun, in the gradations of brightness as it recedes from that orb, and in the relative visual extensions estimated *along* the zodiac and *across* it, would seem to be on those rare occasions when one may stand, during a total solar eclipse, quite within the path of total obscuration. I suppose, however, that no such occa-

sion has yet been distinctly improved, for the purpose above indicated; nor—however probably that circumstance may be the result of a too limited information on my part—have I seen reason to expect that the one just at hand is likely to be so improved, otherwise than incidentally and very imperfectly. It will be impossible for the astronomers, intent as they must be upon telescopic observations, to do full justice to the phenomenon in question, and almost equally impossible for any other man who shall not have anticipated in his reflections the specific aspects to which the attention ought to be essentially devoted.

Before quitting this topic, may I be indulged in making an inquiry that naturally grows out of it? Is not the light which, in a total eclipse of the moon, makes her dark face visible to us, derived, in a greater measure, from this equatorial nebula of the sun, than from the refractive effect of the earth's atmosphere? If the intensity and extent of the zodiacal effulgence shall be detected at the occurrence of the coming eclipse, or by any other means, it may be possible to reply very satisfactorily to this inquiry. I would not unhesitatingly assume that a reply *substantially* satisfactory might not be derived from facts already well known. I must own that, hitherto, I have not even undertaken to speculate concerning the amount of illumination, at the moon's surface, due to the terrestrial atmosphere,—a question which would seem, at first view, to be of moderate difficulty, if only the dispersive and refractive powers of common air are exactly ascertained.

But I pass on to some suggestions respecting a phenomenon of a different class. To observers just within the path of total obscuration, —and perhaps, *very transiently*, to those situated deeply within it,— the telescope will probably reveal a fine thread of light, edging some part of that dark limb of the moon which is in near proximity to the sun's corresponding limb. I infer this probability from a similar aspect,—which may indeed have been observed at other times, and recorded, although I have no knowledge that it has been,—that was witnessed by myself, through an excellent instrument, from the station of New York, on the occasion of the annular eclipse of 1838,—or rather the eclipse which *just failed* to be annular, at that station, on account, possibly, of an irregularity in the moon's outline. In any event, it must be rare that the phenomenon under consideration can be exhibited so strikingly as it was on the occasion alluded to, from the very circumstance of my station being at or near the limiting boundary, upon the earth's surface, of the annular aspects. On that occasion I noticed, several minutes before the time of nearest completion of the ring, the fine cusps of the sun's unobscured crescent prolonged by a hair-breadth line of brightness, totally diverse, in color and intensity, from the sun's disc. As the cusps approached, the line or thread of light in advance

of each, shot round the moon's edge, between them, rapidly, till, at a certain time, the threads from the two met and joined in one,—thus uniting the cusps. At a certain time following the instant of nearest formation of the ring the thread became again disunited, and the reverse phenomena of those just mentioned took place.

In meditating, at the time and occasionally at subsequent times, upon this, to me, surprising phenomenon, I could obtain no glimpse of a solution respecting the probable cause, unless by supposing the existence of a *lunar atmosphere*. It is, I admit, only in one point of view that I can be held excusable for offering these phenomena as proof upon this high and much questioned topic, antecedently to having myself demonstrated by a rigid process the mode in which a lunar atmosphere implies and accounts for just those appearances which I witnessed. But, although I am not without my reasonings to fortify the conjecture above presented, those are not to my present purpose. An excuse for my boldness, if I need one, may be found in the nature of my present object, which is simply to invite attention to expected and interesting phenomena, on the part of observers among my countrymen who may be favorably situated abroad for devoting to them the requisite attention, as well as on the part of any others to whom these unpretending thoughts may find way and whom they may concern. A. C. T.

13. *Meteors of April* 18–20, 1841.—About 8 P. M. on the 18th of April, 1841, at Vidalia, Louisiana, Prof. Forshey noticed an unusual number of meteors in different parts of the heavens, and on tracing their paths backwards, found that they traversed the constellation *Virgo*. Having commenced precise observations at half past eight, and continued them for three hours, he saw in two hours and a quarter, (forty five minutes being lost in recording,) sixty meteors, of which, all but five, passed within 10° from the common radiant point. These meteors were very unlike those of the August shower; being chiefly without trains, and of a reddish color, few of them of the first magnitude, and the greater number of the third and inferior magnitudes. Their velocities were remarkably equal and gentle; their paths short, and their light first increasing and then waning. Prof. F. determined their radiant point to be in a line drawn from Spica to θ Virginis, somewhat nearer to Spica, about R. A. 198°, S. decl. 8°. The convergent point was therefore in longitude 19°.6, and lat. N. 0°.3, while the observer's motion was towards a point of the ecliptic, in long. 299°. This gives a deflection of the path of the meteors, relatively to the true path of the observer, of 80°.6; and hence their true velocity cannot have been much less than that of the observer, or about sixteen geographical miles per second. This observation of the convergent point of these meteors, Mr. Walker regards

as strongly confirmatory of the cosmical theory of shooting stars, inasmuch as it seems to demonstrate the existence in this group, of a planetary velocity, like that of the December group observed in 1838, (see this Journal, Vol. xxxv, p. 361, and Vol. xxxvi, p. 355,) in a direction normal to the observer's motion, and incapable of resulting from it.—*Proc. Am. Phil. Soc.* ii : 67.

Observations at New Haven.—From 11h. to 12h. P. M. of April 19, 1841, Messrs. F. Bradley, A. B. Haile, and myself, watched in the S. W. quadrant only, in concert with Mr. S. C. Walker and others, at Philadelphia. During this hour, we saw *thirteen* shooting stars, whose paths we recorded on the star-chart. Of these, two exceeded the first magnitude; two equaled the first magnitude; three were of the second; five of the third, and one of the fourth. The average time of visible flight was one third of a second. No definite radiant was observable, but only a general westward tendency. At 0h. 30m. (20th) we began to watch in the sky at large. Clouds soon came over from the west, and by one o'clock A. M. the sky was so much obscured that we were compelled to desist. In this half hour, we saw three meteors in the N; two in the E., and two in the S. No very definite radiant could be determined, but it appeared that the radiant region was then east of the meridian, and about 70° or 80° in altitude. For five nights following, the sky was wholly overcast. It may be worthy of mention that there was a moderate display of the Aurora Borealis on the nights of the 19th and 20th. E. C. H.

14. *Shooting Stars of Dec.* 7, 1838.*—In a paper communicated January 8, 1839, to the Meteorological Society of London, by J. H. Maverly, Esq. of Gosport, he states the following observations:—"On the day after this storm, (of Dec. 2, 1838,) there were showers of hail and rain, two double rainbows, and one lunar rainbow at $6\frac{1}{2}$ P. M. On the night of the 7th, between $7\frac{1}{2}$ and 10, he noticed *ninety seven meteors*, viz. *fifty six* eastward of the meridian, and *forty one* westward of it." * * So great was the display, that Mr. M. says, "had this phenomenon occurred between the 12th and 15th of November, those who maintain the opinion of the annual appearance of *showers of meteors*, would have pronounced this extraordinary appearance to have been their diurnal periodical return."—*Proc. Meteor. Soc. Lond.* i : 9.

In the *Institut* for October 14, 1841, M. Colla states, that at *Parma*, in Italy, on the night of December 7, 1838, during three hours, he ob-

* For observations made in this country and elsewhere, see this Journal, Vol. xxxv, p. 361, and Vol. xxxvi, p. 355.

served *one hundred and fourteen* shooting stars. This fact was announced in his Astronomical Annual for 1840, p. 91.

15. *Determination of Longitude by Shooting Stars.*—It has been stated that Dr. Maskelyne first suggested (in 1783?) the utility of corresponding observations of shooting stars and the larger fire-balls for the determination of differences of longitude. It appears, however, that George Lynn is entitled to the credit of a distinct proposal of this kind, made much earlier, in a paper entitled "*A method for determining the Geographical Longitude of Places from the appearance of the common meteors called Falling Stars*," published in the Philos. Trans. of the Royal Society of London, for 1727, No. 400, p. 351. A suggestion somewhat less comprehensive was made still earlier by Dr. Halley, in his account of a large meteor seen in England March 19, 1719, (Philos. Trans. 1719, No. 360.) He says "a considerable use might be made of these momentaneous phenomena for determining the geographical longitudes of places. For if in any places, two observers by help of pendulum clocks, duly corrected by celestial observation, exactly note at what hour, minute, and second such a meteor as this explodes, and is extinguished, the difference of the times will be the difference of longitude of the two places, as is well known."

16. *Ancient Meteorological Memoranda.*—The following notices are copied from entries made by the Rev. James Pierpont, (minister of the first church in New Haven, Conn.) on the blank leaves of an almanac for the year 1692, (by John Tulley: Cambridge, Mass.: printed by Samuel Green and Bartholomew Green, for Samuel Phillips.) The dates being in the Julian style, must of course be advanced ten days to bring them to our present reckoning. E. C. H.

1692. *Tuesday, February* 23. At night an unusual eastern storm of furious wind and rain began, and continued till Sabbath following. Rivers higher than ever known. Wallingford bridge carried away: Great damage through the country.

Thursday, March 3. The aforesaid storm renewed, and continued for that day.

July 1. Latter end of June, multitudes of caterpillars fell on corn, and did much spoil in some places, but were remarkably checkt with us.

July 4. Excessive hot, and a sore drought about the time.

July 9. Excessive hot again. About the time a severe drought. Indian corn almost spoiled: all signs of rain vanisht in drought.

July 11. Unexpectedly, and without foregoing signs, a long shower, which revived all things languishing before.

July 14. More rain, so that every thing was fully recovered to admiration.

August 11. A plentiful rain.

December 21. In the evening, two *dracones volantes*, [meteoric fireballs,] of unusual dimensions were seen; on the extinguishing of one, a noise like a great gun was heard: both light and noise were affrighting to many.

17. *Description of Russell's Planetarium, with improvements.*—This great orrery is drawing towards its completion. When finished, the zodiac will describe a circle of more than 48 feet.

The celestial sphere is about 4 feet 8 inches in diameter, and contains the Sun, Mercury, Venus, the Earth and the Moon. The superior planets are placed on the outside of the sphere; Jupiter, Saturn, and Herschel, having their satellites revolving around them in their proper order, with their inclinations to the plane of the ecliptic. Saturn has his two concentric rings, with their proper inclination and direction.

This armillary sphere is a beautiful structure, and is an important addition to the orrery first made by Mr. Russell.

The whole machine will weigh about one ton and a half, and is composed chiefly of cast and wrought iron, and brass, with but little wood. It contains about 500 cog-wheels, large and small, principally of brass.

The Earth revolves on its axis, inclined as in nature about $23\frac{1}{2}°$, and remains parallel to itself, exhibiting perfectly the manner in which the changes of the seasons are produced, and the variations in the lengths of the days and nights. The other planets also revolve on their axes duly inclined to the planes of their own orbits, so that the causes of the vicissitudes upon each planet are readily comprehended.

The Moon revolves around the Earth in an orbit duly inclined to the plane of the ecliptic; making ascending and descending nodes, the retrograde motion of which is also given, so that the circumstances under which eclipses of the Sun and Moon happen, are clearly shown. The libration of the Moon is also exhibited.

The Sun is represented by a gilt globe about 15 inches in diameter, revolving in about its proper time.

The primary planets are represented by beautiful glass globes made opake, with some attention to their relative magnitudes and telescopic appearances.

Vesta, Juno, Ceres, and Pallas, are all to be introduced in the machine; their motions and great inclinations being properly represented.

Jupiter, Saturn, and Herschel, will furnish us with their splendid little orreries, either attached or detached; making at the same time their proper revolutions while going around the Sun.

Columbus, Ohio, February, 1842.

18. *Abstract of Mr. S. C. Walker's paper entitled Researches concerning the Periodical Meteors of August and November, read before the Amer. Phil. Soc. Jan.* 1841.—This paper contains—1st, Tabular statements of the relative velocities derived from corresponding observations of the same meteor at different stations, chiefly from Quetelet's Catalogue. 2d. A catalogue of remarkable appearances of shooting stars, also from Quetelet, with additions. 3d. Bessel's position of the earth, in the ecliptic, at the date of the principal November showers. 4th. The convergent points hitherto observed for the relative paths of the meteors of August, and 5th. Of those of November. The term *periodical* is restricted to the meteors, which, at a particular season of the year, tend towards the convergent point for that season. *Sporadic* is applied to the unconformable meteors seen on the same occasions. *Extraordinary* showers of the second table are placed in the former class, and are considered as differing from periodical meteors only in numbers. The convergent point, as far as noticed for the periodical meteors, is not far from the antipode of the earth's *tangential* direction. The average relative velocities in table first, with the known convergent points, for August and November, and other parts of the year, as far as observed, afford on the cosmical theory, the most plausible estimate of the elliptic elements of the orbit of periodical meteors. The well-known formulæ for computing these elements are stated; and the differential formulæ are investigated for computing the probable errors of such elements, arising from errors of the relative velocities and directions derived from the foregoing tables. The most plausible elements of the periodical meteors, are thus found to have their perihelia inferior to that of Mercury, and hence are only seen by us when near their aphelia; the orbits being necessarily very eccentric, or flattened, and their inclinations very great. Since many millions of these bodies are annually encountered by the earth, including chiefly those which move in orbits having small parameters, analogy leads to the inference, that the planetary spaces inferior to Venus, abound in these bodies, of which only a small proportion ever reach the earth's mean distance, or become visible to us. This suggestion of a far greater aggregation of these bodies near the sun, is supported by the analogy of the resisting medium encountered by Encke's comet, which is only sensible at a distance from the sun below that of Venus. Bessel's objections to the theory of the resisting medium, that it is indicated by no other phenomenon in nature, may be in some degree obviated by this analogy; since a very thin, light body, might be sensibly resisted by a great multitude of these small meteors or asteroids, though their effect is insensible on Mercury and the other primaries, owing to their superior mass and density, and as Encke remarks, also insensible on Halley's and Biela's comets,

whose perihelion distances, respectively, correspond nearly with those of Venus and the Earth. It is only necessary to suppose that in some planes these bodies exhibit a greater tendency to the formation of clusters, or possibly of flattened rings, in order to account for anniversary periods of remarkable showers; since the earth revisiting the same plane at the same season of the year, and at the same distance from the sun, may or may not encounter one of these clusters or parts of a flattened ring. But these clusters continuing to move in the same plane, the earth must, if it meet them at all, do so at anniversary periods. On the supposition of a flattened ring, the node having the same radius vector as the earth, these displays might occur for several anniversaries, and then cease for an indefinite period, owing to the motion of the apsides of the ring; till the anomaly which has a radius vector equal to the earth's mean distance, again coincides with one of the nodes of the ring. Hence the connexion between the periods of the second table, as far as regards our knowledge of them is *accidental*, since they depend not on the orbital period of these bodies round the sun, but on the circumstance of the earth's encountering one of these clusters, or planes abounding in them, which is regulated by a law of distribution of these bodies in planetary space, that must always remain unknown, for want of data for its determinatton.

The author conjectures that the meteors termed *sporadic*, by Quetelet, which have no common convergent point, may have their perihelia superior to those of the periodical meteors, and their aphelia far superior to that of the earth. In such a case, their orbital velocity would be as great as that of the earth, or greater; and as they move in all varieties of direction, the earth's tangential motion does not cause them to tend, relatively towards a convergent point, in nearly an opposite direction, as it does with meteors moving very slowly in their orbits, whatever may be their true directions in space.

A brief history of the opinions and theories of writers on this subject is given; and an oversight pointed out in Prof. Erman's paper, quoted by the author in an oral communication of August 21st, 1840. This relates to Prof. Erman's minimum relative velocity of the meteors, which, instead of being 0.83, of that of the earth, may be indefinitely small, and therefore in his formulæ [Artronomische Nachrichten, No. 385, p. 9,] may give a motion of the convergent point indefinitely great. The author also remarks, that the quantities neglected in Prof. Erman's formulæ for this motion, may produce an important effect on the result, and even change its direction from a retrograde motion, as found by Prof. Erman, to a direct motion as observed by Mr. Fitch, at New Haven, and as indicated by Prof. Forshey's observed positions of this point at two different dates on the night of the 10th of August last.—*Proceedings of the American Philosophical Society, Feb.* 1841.

19. *Barometric Minima of February* 16–19, 1842.—During the violent gale, which swept along the coast of the United States between the 15th and 20th of February last, the oscillations of the barometer, were very extraordinary and perhaps unprecedented. In Boston, the following were the observed altitudes of the mercury in that instrument, reduced to the temperature 50°, to the mean level of the sea, and to the true level of the cistern.

Feb. 15,	10*h*.	30.36	
" 16,	13	28.47	fall 1.89 in 27 hours.
" 17,	19	30.39	rise 1.92 in 30 "
" 18,	2	30.39	stationary 5 hours.
" 19,	2	29.46	fall 0.93 in 24 hours.
" 20,	2	30.43	rise 0.97 " "

Amount of oscillations 5.71 inches in 4 days 11 hours.

The least height I had ever previously noticed in Boston, occurred Jan. 1st, 1827, viz. 28.62, and the greatest on Jan. 1st, 1839, 31.11. From the above, it appears that the extreme range in Boston, in the course of many years is 2.64 inches, nearly three quarters of which were *twice* experienced in 57 hours between Feb. 15th and 17th last.

Boston, March 7, 1842. R. T. P.

At New Haven, Conn., the barometric minimum occurred Feb. 16th, 10*h*. P.M., the column, when reduced as above, standing at 28.69 inches. During the day the gale blew from S. 62° E.; on the 17th, from N. 88° W.—Eds.

20. *Meteorite of Château-Renard.*—A fragment of the meteorite which fell near Château-Renard, in France, June 12, 1841, has been examined by M. Dufrénoy. The meteorite appears to have burst, at an elevation which cannot be determined, into several pieces, of which two only were seen to fall on the earth, about forty paces apart. One of these pieces falling on a rock was broken into a multitude of small fragments; the other buried itself to a depth of about 20 centimetres, (8 inches,) and has separated into but a few fragments, of which the largest is 35 centimetres (14 inches) long, and 11 centimetres (4½ inches) wide.

The exterior of this stone is covered with the black crust which is observed on all meteorites. Its fracture is granular. A small vein traverses the whole mass. Externally this meteorite resembles trachyte; it is of a clear gray, and is composed entirely of crystalline portions, which cross each other as in the volcanic porphyries. However, the spherules of metallic iron, which are scattered with much uniformity, throughout the mass of the stone, indicate a different nature from that of any terrestrial product, for iron is not found here in a metallic state:

at least its discovery has been alleged, and that in a doubtful manner, in but three or four localities. This aerolite resembles, on the contrary, in a remarkable manner, some of the slags of the furnace. Examined with a strong magnifier, two distinct minerals are recognized: one imperfectly lamellar, presents in some parts bands analogous to those which characterize the hemitropic masses of albite or labradorite: the other, of a vitreous fracture, might be taken for quartz, if we did not know from numerous observations, that this mineral is not found in true volcanic rocks, nor in those of meteoric origin. Besides these two minerals, the eye detects small black glassy globules, analogous to perlite. These are evidently the product of fusion, and their gray interior, which is like the general texture of the stone, has not been altered by the heat. Finally, there are to be detected small shining black plates, which are particularly collected about the veins which traverse the stone. These small plates resemble the scales of graphite which exist in some varieties of gneiss. The gravity of the stone is 3.56: that of the grains of metallic iron, extracted by the magnet, is 6.48.

Before the blowpipe, a fragment is immediately reduced to a black hollow scoria, like that of the exterior crust of the stone. This proves that the crust is the result of the fusion of the exterior parts, which are oxidized to a very high degree by their contact, when at an elevated temperature, with the atmosphere.

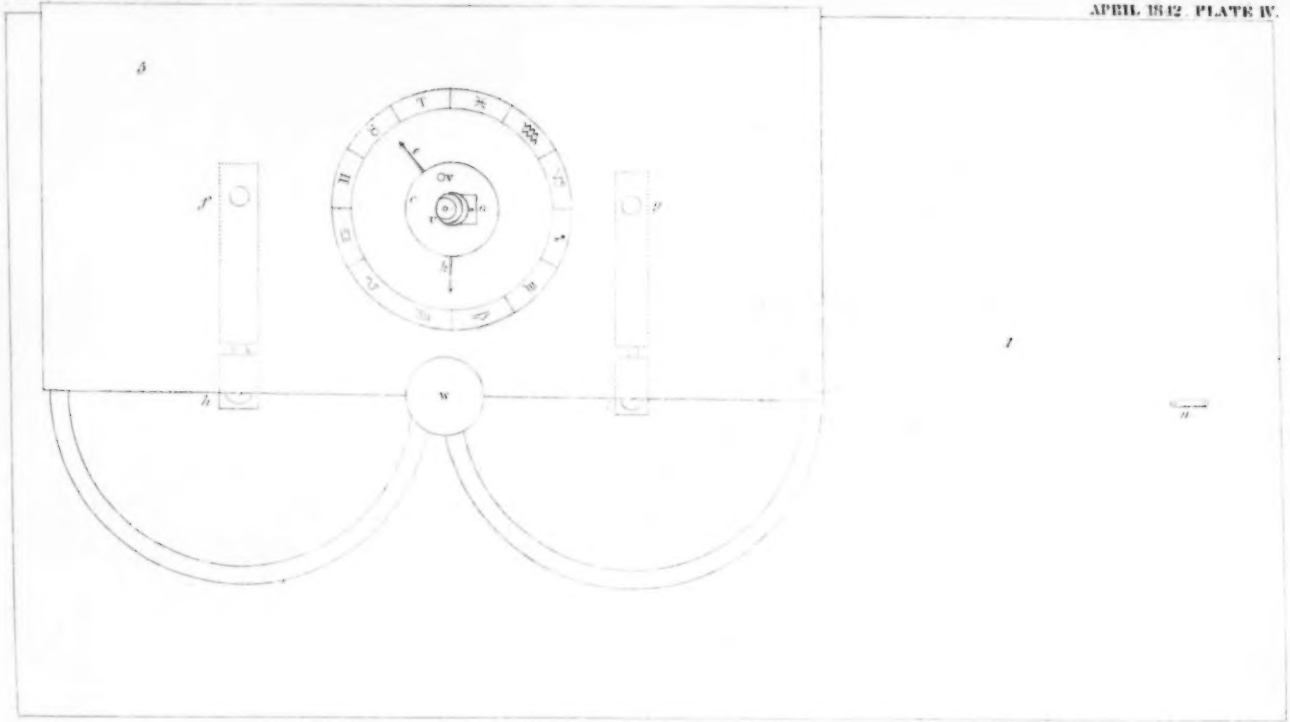
M. Dufrenoy gives the following as the result of three analyses which he made.

Silica,	38.13
Magnesia,	17.67
Protoxide of iron,	29.44
Protoxide of manganese,	a trace.
Alumina,	3.82
Lime,	.14
Metallic iron,	7.70
Nickel,	1.55
Sulphur,	.39
Potassa,	.27
Soda,	.86—99.97

Or, grouping together the elements which are combined:

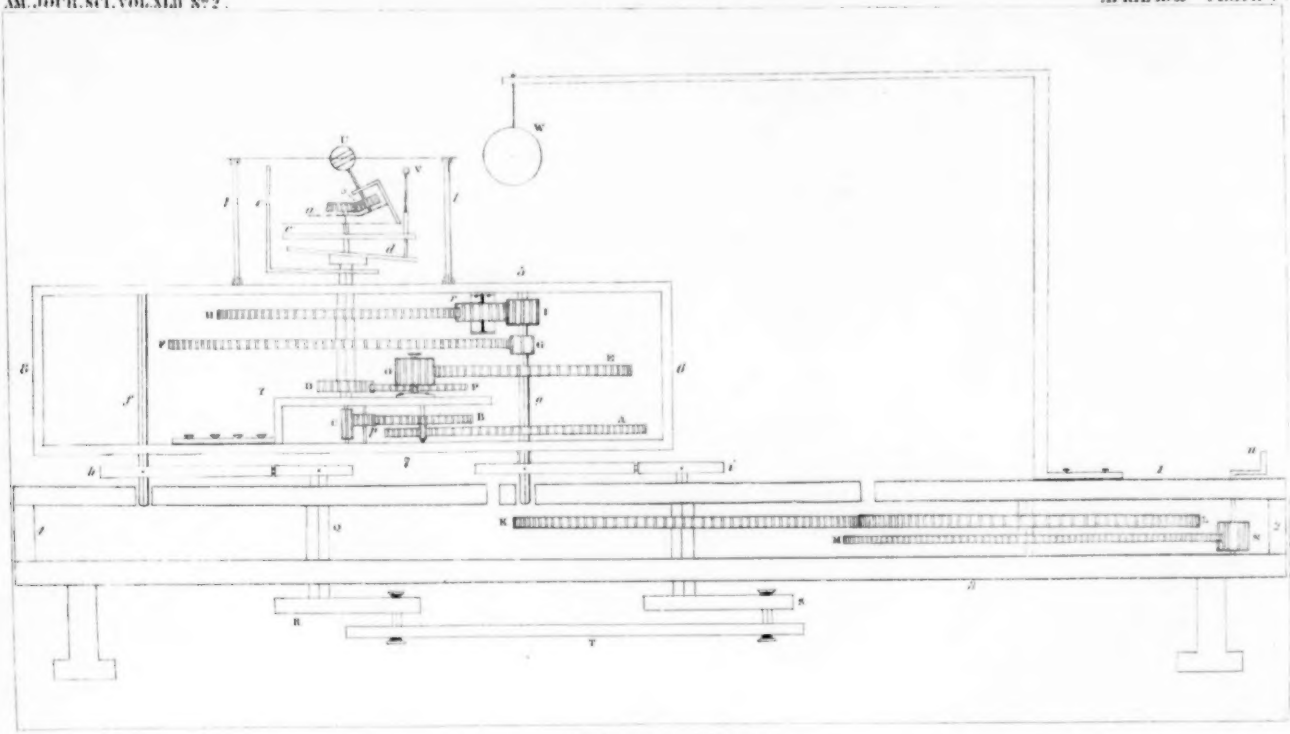
Alloy of iron and nickel,	9.25
Pyrites,	.67
Ferruginous peridote, soluble in acids,	51.62
Matter insoluble in acids, and not related to any known mineral,	38.17—99.71

L'Institut, July 22, 1841, *No.* 395.



THE TELLURIUM.

An Astronomical Machine by Edwin C. Leedom, M.D. Plymouth, Pa.



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